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The Challenge of Modern Historiography

BERNARD BAILYN

GORDON WRIGHT, speaking from this rostrum a few years ago, warned that those who have the honor of perpetuating the Association's ritual of presidential addresses "might do well not to take their pronouncements as the voice of God or the crystallized wisdom of the ages," and he wondered if it were not significant that the president is allowed only one parting shot to speak *ex cathedra*, "not at the outset of his term of office but at the very end, only forty-eight hours before he 'passes into history,' as the saying goes. By that time it is much too late for him to make promises, to influence the Association's future course, or even to be held to answer for his stewardship or for such sophistries as his swan song may contain." Having thus taken the curse off any *ex cathedra* pronouncements that might follow, Professor Wright proceeded to pronounce on one of the most elevated, difficult, and controversial issues that faces historians who think about what they do—namely, the degree to which history is a moral science.¹ I admire his courage, but I take my lead from his warning. What follows is nothing more than a general consideration of certain problems of modern historiography encountered by a working historian—a historian, as it happens, just emerging from a considerable period of research and planning for a large-scale project. This project is an effort to describe as a single story the recruitment, settlement patterns, and developing character of the American population in the preindustrial era. It covers a long period of time—the two hundred years from the early seventeenth century to the advent of industrialism. Further, it involves population movements over a vast geographical area—an area stretching from the bleak island of Foula off the west coast of the Shetlands at the latitude of Greenland to the Lunda Kingdom deep in equatorial Africa, from the Baltic port of Flensburg and from Görlitz on the German-Polish border to Natchez and Pensacola. And, finally, the problems it involves lead naturally beyond history itself to other disciplines as they relate to history:

This presidential address was delivered at the Ninety-Sixth Annual Meeting of the American Historical Association, held in Los Angeles, December 28–30, 1981. I presented some initial thoughts on the themes of this address at a conference on History in the 80s, held in Bellagio, Italy, June 1980. I would like to thank Barbara DeWolfe for her expert and diligent help on the research that is reflected in this paper. I am indebted, too, to several colleagues who read and criticized the paper in draft.

¹ Wright, "History as a Moral Science," *AHR*, 81 (1976): 1.

anthropology, demography, and, particularly, cultural geography. Yet, despite the breadth of this project, I am painfully aware that any general statements I make about contemporary historiography and its problems as a whole are severely limited by my knowledge, by my primary emphasis on Anglo-American history of the early modern period, and by the kinds of studies I happen to have made in the past and am engaged in now.

My emphasis on the early modern period of North American and Western European history does, however, have an advantage. In recent years this transitional period between our distant and our immediate past has enjoyed an extraordinary growth in scholarship. This segment of historiography has simply exploded since World War II, and, instead of subsiding after great tumultuous blasts, the explosions continue. Books and articles on the three hundred years after the European discovery of America drop from the presses in heaps, and essays of general interpretation multiply endlessly. The topics of current interest cannot easily be catalogued. Anyone interested in the whole range of innovative scholarship in the early modern history of the Western world is involved in the latest refinements in the study of the discoveries and explorations, in parish records of France and England, in family, community, and demographic studies from everywhere from Uppsala to Florence, in the evolution of royal courts, state offices, and parliamentary bodies, in mobility patterns and migrations, in the everyday lives of workers and witches, in race conflicts, social stratification, the uses of leisure, sex practices, burial customs, magic, *mentalités* and ideologies of all kinds, and attitudes to everything: to birth, to life, to work, to age, to death, and to life after death. Only a besotted Faust would attempt to keep up with even a large part of this proliferating literature in any detail.

What is happening in this area of contemporary historiography is distinctive, I believe, in its magnitude, variety, and speed of growth; but in lesser degrees the same thing is happening elsewhere. Modern historiography in general seems to be in a stage of enormous elaboration. Historical inquiries are ramifying in a hundred directions at once, and there is no coordination among them. Even if one reduces the mass of new writings in the early modern period to the American field, and still further to the publications of card-carrying historians, the sheer amount of the writing now available is overwhelming. But limitations like that are arbitrary. Fields and problems that were once discrete and rather easily controllable merge, lose definition, reveal depths below depths. Early American history, once a neatly delimited field of study, seems now boundless; it is incomprehensible in isolation from Western European and African history. Further, some of the most interesting studies within it are being carried out by scholars in other disciplines: geographers, who find in historical data a rich field of inquiry; economists who are interested in the developmental aspects of the creation and distribution of wealth; methodologists, who are mainly concerned with perfecting techniques, largely quantitative, for inquiries in a broad range of social sciences; theologians and philosophers whose studies are rooted in the great texts of this period; and anthropologists and sociologists who understand the fundamental importance of time development for their own proper work. In other fields of history, too, nonhistorians, whose studies

bring them into contact with the records of the past and whose view of their own subjects gives heavy weight to development through time, contribute steadily to the bulging dimensions of history as it is now written. We learn from them, they learn from us; paths cross and identities merge, and historiography grows ever broader—and, one would have thought, deeper and more meaningful. But depth of understanding is a function, at the least, of coherence, and the one thing above all else that this outpouring of historical writing lacks is coherence.

THE GREAT PROLIFERATION OF HISTORICAL WRITING has served not to illuminate the central themes of Western history but to obscure them. The most venerable structure of Anglo-American history known in its narrowest form as the “whig” interpretation—political in essence but fleshed out with social and economic history—which explained the present in terms of an inferior but improving past, has long since been so severely eroded that the turning points and the overall contours of the story have almost entirely disappeared; and no new general interpretation or approach of equal comprehensiveness has developed in its place. A few isolated struts are left here and there, while ever more learned detailed studies pile up haphazardly all around. For a time it seemed that in the area of social history the concept of “society” had become a general organizing principle.² It promised to transform the traditional, loosely descriptive *Sittengeschichte*—“a disorganized mass of half truths,” it was once called, “dealing as it does with a sort of chaos of habits and customs, ways of living, dressing, eating, and the performance of the duties of existence”³—into a sharply focused explanation of how traditional Western society of the late medieval period evolved into the modern social order we know. That concept remains essential to such coherence as social history now has. But studies of aspects of “society” in the past—classes, estates, communities, families—have now so increased that the subject, even within that definition, seems to be beyond comprehensive control. Detailed community studies multiply with such speed in so

² E. J. Hobsbawm, “From Social History to the History of Society,” in M. W. Flinn and T. C. Smout, eds., *Essays in Social History* (Oxford, 1974), 1–22.

³ Charles M. Andrews, “On the Writing of Colonial History,” *William and Mary Quarterly*, 3d ser., 1 (1944): 31–33. Andrews’s essay, a posthumously published apologia for his prodigious accomplishments in Anglo-American history, centers on the conceptual problems of social history. Andrews described his framing ideas in writing *The Colonial Period of American History* (1934–38) and then explained that he had intended to go beyond these four volumes focused on the institutional “framework of constituted authority” to a volume on “colonial life in the eighteenth century, . . . an *omnium gatherum* of everything not political, institutional, or military.” But how was such a volume to be organized? He confessed himself perplexed. He could see no structure to the subject in acceptable historical terms—that is, as development, process, evolution. Two approaches seemed possible, neither acceptable. On the one hand, the subject could be conceived of as “what is vaguely called the ‘social sciences’”; but history, he wrote, is not science: “‘scientific’ treatment always tends toward over-rigidity and a mechanical interpretation of the subject matter that takes no account of the baffling complexity of the human equation, and ignores—what cannot be ignored—the inevitable presence of much that is casual and inexplicable.” On the other hand, it could be seen simply as the chaotic accumulation of “habits and customs, ways of living, dressing, eating, and the performance of the duties of existence.” And that, too, was unsatisfactory, having neither structure nor development. Some synthetic, structural, and above all developmental theme was needed, he realized, and the best he could offer was that of “a progressive movement . . . indicative of what may be called an Americanizing process.” It was precisely the concept of “society” that he lacked, and his struggle to extemporize some approximation to that conception is instructive.

many places based on such disparate data that synthesis into a coherent whole, even for limited regions, seems almost impossible. The latest writer on the subject states flatly that “the intensive study of early modern European social history by the current generation of historians has brought forth no general agreement—and very little theoretical analysis—of the social structure of pre-industrial Europe.”⁴ One grasps, firmly, a methodologically splendid instance—a solid little piece of the beast, but whether of its nose or its tail one does not know. Historians seeking to understand something larger than the painfully assembled local example sensibly attempt generalizations by bringing together an array of other, local examples; but the empirical “base” is usually thin enough to be quickly undermined by other studies using somewhat different data or simply by reinterpretations of the original data.⁵

Yet, if the proliferating information, much of it quantitative, generated by inquiries into aspects of past societies produces no coherent whole, it does seem to induce a wonderful euphoria. The mere glimpse of the great possibilities of quantitative analysis, which enables one to analyze the characteristics of whole populations and of social structures in times past, leads to dizzying visions of rewriting the whole story of man’s past.⁶ The vision tends to fade, however, with the discovery that the range of inquiry is ultimately limited by the very quantitative techniques that made it possible in the first place, and that the comfort of the apparent clarity, precision, and definitiveness of numbers stimulates the production of ever greater mountains of information, more and more difficult to scrutinize critically and bring into a coherent whole. A poignant moment in modern historiography was reached recently when an encomiast of the *Annales* school, contemplating in rapt admiration Fernand Braudel’s adaptation of Lévi-Strauss’s three-level general communications theory, concluded that in the end, when the whole business was brought up to date and put into historiographical operation, there would be “16,777,216 subsystems”—no big job for a decent computer to handle, the author assures us. But one small problem remains: “who,” he asked, “would read the enormous number of printouts?”⁷

Braudel: —everyone knows of Braudel’s truly heroic attempt to introduce an olympian principle of coherence into the vast mass of historical documentation. His aim was to write a “total” history of an entire “world”—to include everything from pots and pans to politics and from geological foundations to cultural achievements. He sought to do this by grouping the affairs of mankind into events of three distinct

⁴ Armand Arriaza, “Mousnier and Barber: The Theoretical Underpinning of the ‘Society of Orders’ in Early Modern Europe,” *Past & Present*, no. 89 (1980): 39.

⁵ See, for example, James Henretta’s interesting effort to generalize across the first group of early American community studies—Henretta, “The Morphology of New England Society in the Colonial Period,” *Journal of Interdisciplinary History*, 2 (1971): 379–98—and the criticism of that effort, based on new data from a region only a few miles to the north, just then being assembled—Darrett B. Rutman, “People in Process: The New Hampshire Towns in the Eighteenth Century,” *Journal of Urban History*, 1 (1975): 268–92. For a complete inversion of conclusions reached by authors of notable recent community studies, see W. R. Prest, “Stability and Change in Old and New England: Clayworth and Dedham,” *Journal of Interdisciplinary Studies*, 6 (1976): 359–74.

⁶ Thus, Arthur Imhof hoped to see the whole of German social history rewritten through a massive reconstruction of family and community life over the past four to five hundred years; see his “Historical Demography as Social History: Possibilities in Germany,” *Journal of Family History*, 2 (1977): 305–22.

⁷ Traian Stoianovich, *French Historical Method: The Annales Paradigm* (Ithaca, N.Y., 1976), 100.

time dimensions, dealt with separately in three books bound in one: events that are deeply circumstantial and most slowly moving (climatological and geographical history); the more rapid movements of change in social structure and economic patterns; and the swift, hectic, day-by-day movements, the “nervous oscillations,” of men in action—that is, politics. He wrote his famous three-layered book on the Mediterranean in the sixteenth century as if the interpenetration of the spheres would somehow happen automatically. In fact, it happened only to the extent that Braudel violated his own abstract scheme. Braudel’s *Méditerranée* has been justly celebrated for its wealth of information, much of it esoteric, and its revealing, at times brilliant, descriptions of the way people lived; but it should be known too for its ahistorical structure, which drains the life out of history. For the essence and drama of history lie precisely in the active and continuous relationship between the underlying conditions that set the boundaries of human existence and the everyday problems with which people consciously struggle. The goal of history is not to separate out events of these different dimensions at a particular point in time but to show their continuous interaction in an evolving story. The drama of people struggling with the conditions that confine them through the cycles of limited life spans is the heart of all living history, and the development of that drama itself, not a metahistorical scheme of classifying events, must provide the framework for any effective interpretation of history.⁸

There have been other kinds of efforts to bring aspects of the growing mass of historical data into some degree of meaningful order. At a few centers of historical research, data bearing on particular ranges of problems have been systematically collected and conflated, studies have been undertaken to fill in evident gaps, and reports of various dimensions have been issued summarizing the state of the inquiry as it develops. But this kind of systematically cumulative and cooperative research is in fact rare, and it is in its nature preparatory. All of this work will ultimately prove to be only as important as the historians can make it who will one day use the results of this team research together with all the rest of the available evidence to write, not research reports, but history—that is, narrative accounts of large segments of the general story that help explain how the present world came to be as it is.

The Marxists, of course, have introduced a powerful framework of coherence into historical writing. Whatever their weaknesses, Marxist historians did seek—do seek—above all to relate conditions, circumstances, to the struggles and achievements of mankind and to bring together materials from all sides into a single coherent account of how the present emerged from the past. They see underlying forces shaping men’s lives fundamentally, either directly or through “dominant ideologies,” and have sought to depict both the basic forces and the structures of

⁸ See Bailyn, “Braudel’s Geohistory—A Reconsideration,” *Journal of Economic History*, 11 (1951): 277–82. The literature on Braudel is, of course, voluminous. For a general discussion of his writing, see the *Journal of Modern History*, 44 (1972): 447–539. For some general thoughts on the *Annales* phenomenon, see Bailyn, “Review Essay,” *Journal of Economic History*, 37 (1977): 1028–34. For an explanation of why the *Annales* historians, led by Braudel, have concentrated on “motionless history” (“histoire immobile”) at the expense of historical evolution and the process of change, see H. L. Wesseling, “The *Annales* School and the Writing of Contemporary History,” *Review* [Binghamton, N.Y.], 1 (1978): 185–94. Cf. Emmanuel LeRoy Ladurie, “Motionless History,” trans. John Day, *Social Science History*, 1 (1977): 115–36.

social and cultural life within a comprehensive scheme that concentrates on critical transitions. The Marxists' vision remains a powerful force in our awareness of the past, whatever our approach to history happens to be. But long before the present explosion of social data, their scheme proved to be too inflexible to encompass the vast array of available historical information; they could not allow sufficiently for the shaping role of ideas, of individual decisions, and of accident; and their conclusions have too often been shown to be wrong. The more technically strict the Marxist interpretation, the less comprehensive the coverage of the data is likely to be; the more comprehensive the coverage, the less strictly Marxist—the more diffuse—the interpretation will be. We are all Marxists in the sense of assuming that history is profoundly shaped by underlying economic or “material” configurations and by people's responses to them; few of us are Marxists in the doctrinal sense of believing that these forces and these responses alone are sufficient to explain the course of human affairs.

But the absence of effective organizing principles in modern historiography—its shapelessness, its lack of general coherence—is not simply the result of the immense increase in writing. It stems, I think, from deeper roots. Many of the most energetic historians have forsaken the general goals of history for technical problem-solving, and not for trivial reasons. Anyone who has struggled with the mind-absorbing, soul-entrapping difficulties of subjecting scrappy social data of the pre-statistical era to computer analysis will know how captivating and strangely satisfying, yet how severely vision-limiting, that kind of technical work can be. Absorption in the fascinating technical problems of history is no new thing. It is as old as modern professional scholarship. It happened first, perhaps, to some of the most gifted nineteenth-century historians of the ancient world for whom epigraphical, prosopographical, and legal studies—ever more sophisticated and demanding—became ends in themselves, addressed with increasing elegance and rigor to a decreasing audience of experts. There was only one Mommsen (until Syme) who could advance general historical understanding in a large-scale history and at the same time dominate the world of technical analysis. It happened later, differently, to the Namierites, who tackled with brilliant success problems in the organization of politics in eighteenth-century Britain that few historians had ever glimpsed before. But for all their technical skill Namier and his small group of followers failed to keep their carefully collected data on the minutiae of political “interest” in balance with the evidence of the beliefs that swayed men's minds and the larger allegiances that overrode the ubiquitous factions. Namier understood, correctly, that his technical studies of the organization of politics would recast British historiography, but he never knew precisely how, or what the dimensions of this new historical world would be, since he wrote learned research reports, monographs jammed with quotations and studded with citations to manuscripts, and essays of vast sweep, but not the general narrative history within which his technical analyses fell. He never saw, therefore, the boundaries of the picture he sought to compose, the limits of the hitherto unsuspected truths he unearthed; nor did his followers, who confidently and incautiously extended his analysis backward in time.⁹

⁹ J. H. Plumb, *The Growth of Political Stability in England, 1675–1725* (London, 1967), xiv, xv. Namier was aware of the problem. He had originally intended to write a narrative account of the British “political

There is something about the advancing movement of historical scholarship that induces this periodic absorption of creative minds in technical problem-solving—an alternating dipping and soaring motion of the mind as it drops down to scrutinize puzzling, tangled details, then struggles, not always successfully, to rise again to view the landscape whole. Perhaps that is the way historical understanding must grow. But, whether or not that is so, large areas of history, including some of the most intensively cultivated, have become shapeless, and scholarship is heavily concentrated on unconnected technical problems. Narratives that once gave meaning to the details have been undermined and discredited with the advance of technical scholarship, and no new narrative structures have been constructed to replace the old. Few historians even attempt now to incorporate the mass of technical findings and the analytical studies that dominate modern research into historical narratives that explain how the world—or some large segment of it—evolved in the way it did. Yet the historical meaning, the relevance and significance, of the technical writings can only be found within and as part of such comprehensive, developmental accounts.

To write such essential narratives—dominated by a sense of movement through time, incorporating the technical studies, and devoted to showing how the present world was shaped by its emergence from a very different past and hence concentrated on critical transitions from the past toward the present—seems to me to be the great challenge of modern historical scholarship. We will continue to need, and will continue to have, innovative technical studies; they extend the range of our knowledge and emerge naturally from the inner propulsions of professional scholarship. And we will need and will continue to have analytical works that explore key issues, personalities, and events in depth. But the critical need, it seems to me, is to bring order into large areas of history and thus to reintroduce history in a sophisticated form to a wider reading public, through synthetic works, narrative

nation” during the American Revolution, concentrating on “that marvellous microcosmos,” the British House of Commons. But he found that if he attempted a narrative, “however much of general analysis I put into the introductory chapters, lengthy digressions and appendices could not be avoided—too much in eighteenth-century politics requires explaining.” So, instead, he wrote the two-volume *Structure of Politics at the Accession of George III* (1929), a static analysis of eighteenth-century politics as it existed at one point in time. He continued to believe that this magisterial book was, “in a way, introductory to my main work”; *Structure of Politics*, 1: vi, vii. Later his wife and biographer explained Namier’s perception of the problem he faced in apt metaphorical terms. Namier, she wrote, saw two choices open to the historian. “The one he likened to following a stream as a diarist on the move might, noting day by day its twists and turns from the source, say in the hills, through barely billowing land to the delta that fans out to the sea. The other was to build across the river’s course two dams and settle down to study that section’s significant detail; which study should include analyses of the water and the river bed. Such was his choice in 1924,” when he started out on *The Structure of Politics*. Julia Namier, *Lewis Namier, a Biography* (London, 1971), 187.

It was this sacrifice of narrative to structure, as well as Namier’s rejection of beliefs and ideas as forces in history, that stimulated Herbert Butterfield’s bitter attacks on him; see especially Butterfield, *George III and the Historians* (London, 1957), bk. 3, sec. 3. In criticizing Namier personally for his handling of political history, Butterfield anticipated the general problem of historiography that developed later and that is the main theme of this essay. “Over and above the structure of politics,” Butterfield wrote, “we must have a political history that is set out in narrative form—an account of adult human beings, taking a hand in their fates and fortunes, pulling at the story in the direction they want to carry it, and making decisions of their own. We must have the kind of story in which (no matter how much we know about the structure of politics and the conditions of the time) we can never quite guess, at any given moment, what is going to happen next. . . . Perhaps the ideal kind of history is the kind in which a story is given and events are presented in motion, but the story is re-told so to speak ‘in depth,’ so that it acquires a new dimension; it is both structure and narrative combined. . . . Where history is both a story and a study, one may gain a profounder insight into both the ways of men and the processes of time.” *Ibid.*, 206–07.

in structure, on major themes, works that explain some significant part of the story of how the present world came to be the way it is.¹⁰ There is no prescription for such works, no obvious list of themes of appropriate magnitude. The most successful such narrative I know of is Ronald Syme's *The Roman Revolution*, which retells the story of the great transformation of the Roman state and society between 60 B.C. and A.D. 14 in terms of "the composition of the oligarchy of government." It is a narrative, political in a complex social sense, that had to be drawn from a body of technical studies of family history so "overwhelming" in bulk, so recondite and detailed, that, Syme wrote, it "almost baffles exposition."¹¹ Such narratives may develop in intellectual or economic history as well as in socio-political history; they are most likely to develop, as Lord Acton understood and as Oscar and Mary Handlin demonstrated in considering the history of liberty, in a combination of areas.¹²

These narrative histories will be difficult to write insofar as they incorporate a range of technical, analytical findings. Their structure and the orders of events they describe will follow no standard form. And the difficulties will be compounded by the growing importance of certain broad tendencies, certain inner movements, that are developing within the mass of current scholarship without respect to field and that seem to be creating new dimensions altogether. These developments were not planned. They reflect no methodological doctrine, historical school, or program of research. They have emerged in many subject areas simultaneously, impelled by the dynamics of scholarship itself—by the stimulating effect on sensitive minds of the great increase in documentation in familiar fields, by the influence of ideas developed in other disciplines on historians seeking new approaches and deeper understanding, and by opportunities for new departures suddenly glimpsed by young historians in areas left behind by the once innovative work of older historians. But, whatever their origins, these trends in current scholarship are important in themselves, worth isolating and worth examining particularly for their bearing on the general narratives concentrated on major transitions that will, one hopes, eventually be written.

And so, mindful of Gordon Wright's warning not to mistake one's own observations for the voice of God or the wisdom of the ages, and with apologies in advance for the severe limitations of my unbalanced emphasis on certain kinds of Anglo-American studies only marginally extended into Continental European history, I would like to sketch these trends as I see them emerging from recent

¹⁰ I agree with much in Lawrence Stone's article, "The Revival of Narrative," which I read after drafting this essay. But while, in tracing a resurgent interest in narrative history of all kinds, he was cautiously attempting only to "chart observed changes in historical fashion," I am incautiously doing what he so carefully avoided, making "value judgments about what are good, and what are less good, modes of historical writing" in certain circumstances; I am attempting to suggest why, in this era of great advances in highly professional, highly technical scholarship, narration, accessible to a broad public, is peculiarly necessary. Further, I am more concerned than he seems to be about the sheer disarray and confusion in the proliferation of analytical historiography. Finally, I do not think of narrative history, in the broad sense I am using that term, as incompatible with analytical history. In my view, it is the goal of narrative history, of the dimension now called for, to incorporate the analytical findings. Stone, "The Revival of Narrative: Reflections on a New Old History," *Past & Present*, no. 85 (1979): 3–24.

¹¹ Syme, *The Roman Revolution* (Oxford, 1939), vii, viii.

¹² Handlin and Handlin, *The Dimensions of Liberty* (Cambridge, Mass., 1961); Gertrude Himmelfarb, *Lord Acton: A Study in Conscience and Politics* (London, 1952), chap. 6, esp. pp. 142, 145.

historical scholarship—trends that are likely to shape any comprehensive narratives that reflect the knowledge and analytical skills we now have.

THE FIRST BECOMES CLEAR through a consideration of the importance of quantification. Quantification in history is easily misunderstood. As David Herlihy has explained, it is distinct from computation and the formal analysis made possible by computers. Much confusion has resulted from the failure to observe this distinction.¹³ Further, as Oscar Handlin and others have shown, if it is not practiced with careful discrimination and by historians otherwise informed of historical reality, it can destroy the foundations of historical understanding by limiting questions to available numerical answers, by endowing with a spurious rigor claims that have no basis in fact, and by diverting attention from the central themes of an evolving inquiry.¹⁴ But, beyond all of that, the innovations that are claimed for quantification are exaggerated. Historians have always used numbers, when they could get them; they have always attempted to convey magnitudes in numerical as well as verbal terms. Yet there is something in the current euphoric development of quantification in history that is new and that will, I think, greatly affect the future evolution of historiography generally.

Some terms borrowed from Freud and the sociologists but used here in a somewhat different way may help one see the character of the development.¹⁵ It is reasonable, I think, to say that almost all history written before the twentieth century was essentially *manifest* history. That is, history was the story of events that contemporaries were clearly aware of, that were matters of conscious concern, were consciously struggled over, were, so to speak, headline events in their own time even if their causes and their underlying determinants were buried below the level of contemporaries' understanding. And this could hardly have been otherwise since, quite aside from what historians might have thought was important, the available documentation was derived largely from public records, from the personal archives of great men much involved in the headline events of their own time, and from literary accounts of other kinds variously focused on manifest events. Underlying circumstances, however skillfully and imaginatively described, were secondary concerns introduced as prefatory matter or interleaved here and there to help explain the main events, which formed the structure of the story, or to help create a realistic picture of the era in which the events took place. Sometimes these prefatory or contextual descriptions were remarkably effective: Macaulay's third chapter, for example, or Henry Adams's opening six chapters of his *History of the United States*, or David Cecil's fourteen-page pointillist depiction of the social world

¹³ Herlihy, "Numerical and Formal Analysis in European History," *Journal of Interdisciplinary History*, 12 (1981): 115–19.

¹⁴ Handlin, "The Capacity of Quantitative History," *Perspectives in American History*, 9 (1975): 7–26, expanded in Handlin, *Truth in History* (Cambridge, Mass., 1979), chap. 8.

¹⁵ Freud's use of "manifest content" in dream analysis has been taken to mean "the descriptive narrative that the subject puts forward at a time when he does not have the full meaning of his dream at his disposal"; "latent analysis" for Freud was "a description for everything that analysis gradually uncovers"; J. Laplanche and J.-B. Pontalis, *The Language of Psycho-Analysis*, trans. D. Nicholson-Smith (London, 1973), 243, 235. Cf. Robert Merton, "Manifest and Latent Functions," in *Social Theory and Social Structure* (Glencoe, Ill., 1949), chap. 1, esp. pp. 51, 61–81.

of the eighteenth-century Whig aristocracy with which he began his *Melbourne*—one of the most effective vignettes of social history ever written. But, however effective these passages may be, they form an accompaniment to, a commentary on, a background for, the essential foreground, which remains the story of manifest events.

What is new, it seems to me, about the current work in quantitative history is not that numbers as such are being introduced, or more precise numbers than we have had before, but that the kind of numbers being introduced is making possible a new range of inquiry into what might be called *latent* events—that is, events that contemporaries were not fully or clearly aware of, at times were not aware of at all, events that they did not consciously struggle over, however much they might have been forced unwittingly to grapple with their consequences, and events that were not recorded as events in the documentation of the time. No one in the seventeenth-century Chesapeake colonies knew that population growth was slowing in Britain and that labor markets were shifting in ways that contracted the flow of white indentured servants to the colonies; the planters only knew that they found themselves relying more and more on the labor of black slaves. The latent history of population growth in seventeenth-century Britain was uncovered by twentieth-century students of population history using quantitative analysis, who also established the fact that it was only in the mid-1680s, and not before, that blacks formed the majority of the Chesapeake region's labor force.¹⁶ Similarly, no one in the Tuscan countryside ravaged by the Black Death in the mid-fourteenth century associated that fearful manifestation of God's wrath with an earlier population decline that had been in motion for a century before the plague struck Europe. It was David Herlihy who uncovered this latent event, entered it, so to speak, into the record, and associated it with the manifest devastation of the plague; and he could discover this earlier decline only in statistics which he created out of the manuscript tax records of the countryside of Pistoia and the great Florentine survey, the *Catasto*, of 1427.¹⁷

Other examples easily come to mind, especially in connection with population history: shifts in sex ratios, in age at marriage, in birth rates and death rates, in age distributions, and in mobility patterns. But such key events in population history are only the most obvious of this new range of historical episodes. Events of the same order are now being discovered frequently by historians working on quite different questions: occupation and wealth distributions, church membership, patterns of landholding and types of land usage, living arrangements. It is not simply that quantification is making possible a more precise description of these events. The events I am referring to were known, if at all, only vaguely by contemporaries or by previous historians to have *been* events; they are being discovered as particular happenings now for the first time. Taken together, they form a new landscape—a

¹⁶ Russell Menard, "From Servants to Slaves: The Transformation of the Chesapeake Labor System," *Southern Studies*, 16 (1977): 355–90; and H. A. Gemery, "Emigration from the British Isles to the New World, 1630–1700: Inferences from Colonial Populations," in Paul Uselding, ed., *Research in Economic History*, 5 (1980): 179–231.

¹⁷ Herlihy, "Population, Plague, and Social Change in Rural Pistoia, 1201–1430," *Economic History Review*, 2d ser., 18 (1965): 225–44.

landscape like that of the ocean floor, assumed to have existed in some vague way by people struggling at the surface of the waves but never seen before as actual rocks, ravines, and cliffs. And like the newly discovered ocean floor—so rich, complex, and busy a world in itself—the world of latent events can be seen to be part of, directly involved with, the manifest history of the surface world itself. And that is my point.

One of the most important developments in current historiography, it seems to me, is the emerging integration of latent and manifest events. I do not mean simply that a deeper picture of the context of public events is appearing, although that is indeed happening, but that events of one order are being brought together with events of another order. The resulting conflation is beginning to produce the outline of a general history different from what we have known before. Major public events will, of course, remain in their key locations, but when seen in connection with the clarifying latent landscape they appear to occupy rather different positions than heretofore. The American Revolution, for example, transformed American life and influenced the course of events elsewhere in the world. That manifest event will not be obscured by discoveries of events of another order, but explanations of the origins, development, and consequences of the Revolution are beginning to take on quite different forms in the light of latent events that are now being uncovered. For the extraction of quantitative information from records that were never intended to provide such data makes it possible to detect events in the population and migration history of the pre-Revolutionary years that profoundly affected government policy, settlement patterns, and attitudes to authority, all of which helped shape the origins and outcome of the Revolution. How could the treatment of slavery have been uniform throughout the newly independent American states given the different balances of Creoles and Africans that we have recently discovered existed and given the different degrees and forms of assimilation that we now know developed and that have only recently been located with some precision on the chronological map of American history?¹⁸

THE INTEGRATION OF LATENT AND MANIFEST EVENTS was not planned. It was no one's "research design." It is emerging from the inner logic of historiography itself, which is to say, from the convergence of the efforts of many historians working on different problems and with different kinds of materials. Similarly, there is nothing preconcerted or designed in a second general tendency that is now rapidly developing. It concerns spatial relationships rather than the relationship between different orders of events, and it may be approached historiographically.

One of the most remarkable aspects of recent historical scholarship is the speed with which certain key developments have swept through centers of research and among individual scholars throughout the Western world. The study of family history in its modern form is usually thought to have originated with French scholars building on a long tradition of research in demography. The subject was

¹⁸ William W. Freehling, "The Founding Fathers and Slavery," *AHR*, 77 (1972): 81–93; Ira Berlin, "Time, Space, and the Evolution of Afro-American Society in British Mainland North America," *ibid.*, 85 (1980): 44–78.

picked up in England, where David Glass and others had been studying population trends in early modern history but without focusing on the sociological questions probed by the French, and was then developed with remarkable enterprise and imagination—promoted, indeed, with missionary zeal—in Cambridge University. From there it spread to the United States, where some of us had already been considering the same questions of structure and magnitudes and what might be called the social psychology of the family. But lacking quantitative measures or a technique for developing them, we had relied on the earlier, prestatistical writings of the British historians (which they themselves soon thereafter rejected, along with the work of those of us who had been so naive as to believe them) and on studies done by sociologists who had no idea of what had actually happened in the past.¹⁹ Once the signals from abroad became more reliable and a technique for assembling statistical information became available, research in the history of the family took off in America and has now developed, in typical American fashion, into a decentralized, undisciplined, highly idiosyncratic, but creative academic industry.²⁰ All of this cumulating work in family history has most recently reached Germany, whose records—especially the excellent genealogical records, enhanced by the Nazis' extraordinary *Ortssippenbücher* written to document "pure Aryan" blood lines—will make possible a new level of accomplishment in this kind of study.²¹

What happened in family history happened, too, in historical community studies, in the history of population trends, in the study of modernization, in the history of social structures, and in the excavation of the buried details of eighteenth-century political thought. Discoveries in one country, in one scholarly culture, quickly affected scholarship advancing in other countries.²² Students of American history

¹⁹ Bailyn, *Education in the Forming of American Society* (Chapel Hill, 1960), 15–17, 21–29; Peter Laslett, *Household and Family in Past Time* (Cambridge, 1972), 10–11, 11n.

²⁰ For an effort to summarize and interpret the virtual library of writings on Anglo-American family life in the early modern period that has appeared in the twenty years since I attempted a "hypothetical" sketch, based on the information then available, in *Education in the Forming of American Society*, see Vivian C. Fox and Martin H. Quitt, *Loving, Parenting, and Dying: The Family Cycle in England and America, Past and Present* (New York, 1980), pt. 1.

²¹ See Imhof, "Historical Demography as Social History." On the *Ortssippenbücher*, originally projected as thirty thousand volumes of local history tracing kin associations, to establish "pure" blood lines, from the sixteenth century to the present, see John Knodel, "Ortssippenbücher als Quelle für die Historische Demographie," *Geschichte und Gesellschaft*, 1 (1975): 288–324. An excellent example is the *Ortssippenbuch* of the Palatine village of Lamsheim: Heinrich Rembe, ed., *Lamsheim: Die Familien von 1547 bis 1800 . . .*, volume 1 of the Heimatstelle Pfalz's *Beiträge zur Bevölkerungsgeschichte der Pfalz* (Kaiserslautern, 1971). Excerpts from this volume related to the American migration of the eighteenth century have been published in translation by Don Yoder in *Pennsylvania Folklife*, 23, no. 2 (1973–74): 40ff.

²² Communication has become almost instantaneous. So Peter Laslett, visiting the United States after having completed the manuscript of his *Household and Family in Past Time*, was shown the draft of an imaginative monographic article by an American graduate student on the life cycles of Austrian households in the 1760s. He saw the article's relevance immediately, and hastily amended his manuscript to take account not merely of the new information but of a new point of view, a new dimension of the subject, which thereupon became built into the literature in a permanent way. See Laslett, *Household and Family in Past Time*, 21, 71, 150–51. The manuscript was that of Lutz Berkner, published as "The Stem Family and the Developmental Cycle of the Peasant Household: An Eighteenth-Century Austrian Example," *AHR*, 77 (1972): 398–418. Berkner, however, was by no means satisfied that Laslett incorporated into his thinking the main idea of that essay—the idea that, studied as dynamic, living organisms, families were neither extended nor nuclear in structure; at certain phases of their development they were nuclear, at others extended. The basic argument of *Household and Family in Past Time*, Berkner wrote, remained "that the family has been nuclear in most western societies in the past," but the evidence produced in the book proves "nothing of the sort." Berkner, "The Use and Misuse of Census Data for the Historical Analysis of Family Structure," *Journal of Interdisciplinary History*, 5 (1975): 738.

have good reason, for their own proper work, to examine Pierre Goubert's history of the Beauvaisis, John Patten's studies of East Anglian towns, R. A. Butlin's survey of Irish towns, and Gerald L. Soliday's report on Marburg in Upper Hesse;²³ to compare local community controls in Germany, as described in Mack Walker's *German Home Towns*, with those of England described in Peter Clark and Paul Slack's volumes and in John Patten's book on the same subject; to consider Étienne François's account of the lower classes and poverty in the Rhenish court towns together with Olwen Hufton's *The Poor of Eighteenth-Century France* when assessing Alice Hansen Jones's *Wealth of a Nation to Be*;²⁴ to examine as a basis for comparison with their own materials both the publications of Sune Åkerman and others in Uppsala on migration patterns in Scandinavia and the essays on Spanish migration by Magnus Mörner, Peter Boyd-Bowman, and Gilbert Din;²⁵ and to ponder Franco Venturi's many writings on Beccaria's *On Crimes and Punishments* (1764), so popular and somehow "relevant" in late-eighteenth-century America, though it originated as a polemic in the altogether different world of Habsburg Milan, dominated by a hereditary patriciate allied to the nobility and the Catholic Church.²⁶

There is nothing new in kind in this transnational communication and interaction. Historical scholarship has always been an international enterprise. But seldom has communication been as direct and continuous as it now is. And, more important, never, as far as I know, has the availability of comparable information from far-distant areas in itself reinforced so naturally a major analytical concept. For what is emerging from all of this transnational communication of parallel information is not merely a catalogue of differences and similarities and not simply a progressive sophistication of technique by the application of many minds working in different traditions on similar problems, but something more important: the sense of large-scale systems of events operating over various areas. A rescaling of perspective has begun to take place in which the basic unit of discussion is larger than any of the traditional units within which research began. Large-scale orbits developing through time have become visible, and within them patterns of filiation and derivation.

Since my interests focus on the Anglo-American world in the early modern period, I naturally became aware of this kind of configuration in that connection.

²³ Goubert, *Beauvais et le Beauvaisis de 1600 à 1730*, 2 vols. (Paris, 1960); Patten, ed., *Pre-Industrial England* (Folkestone, Kent, 1979), chaps. 2, 6, and *Rural-Urban Migration in Pre-Industrial England* (Oxford University, School of Geography, Research Paper no. 6, 1973); Butlin, ed., *The Development of the Irish Town* (London, 1977), chap. 3; Soliday, "Marburg in Upper Hesse: A Research Report," *Journal of Family History*, 2 (1977): 164–68.

²⁴ Walker, *German Home Towns: Community, State, and General Estate, 1648–1871* (Ithaca, N.Y., 1971); Clark and Slack, eds., *Crisis and Order in English Towns, 1500–1700* (London, 1972), and *English Towns in Transition* (Oxford, 1976); Patten, *English Towns, 1500–1700* (Hamden, Conn., 1978); François, "Unterschichten und Armut in rheinischen Residenzstädten des 18. Jahrhunderts," *Vierteljahrsschrift für Sozial- und Wirtschaftsgeschichte*, 62 (1975): 433–64; Hufton, *The Poor of Eighteenth-Century France, 1750–1789* (Oxford, 1974); Jones, *Wealth of a Nation to Be: The American Colonies on the Eve of the Revolution* (New York, 1980).

²⁵ Harald Runblom and Hans Norman, eds., *From Sweden to America: A History of the Migration* (Minneapolis, 1976); Mörner, "Spanish Migration to the New World prior to 1810: A Report on the State of Research," in Fredi Chiappelli et al., eds., *First Images of America: The Impact of the Old World on the New*, 2 (Berkeley and Los Angeles, 1976): 737–82; Boyd-Bowman, "Spanish Emigrants to the Indies, 1595–98: A Profile," *ibid.*, 723–35; Din, "Spanish Immigration to a French Land," *Louisiana Review*, 5 (1976): 63–80.

²⁶ On the circulation and reception of Beccaria's essay, see note 39, below.

My first inkling of what would develop in this aspect of historical study came over twenty-five years ago in casual conversations with a colleague expert in the Scottish Enlightenment. It became apparent to us as we talked not simply that the leaders of Revolutionary America and of Enlightenment Scotland shared certain ideas but that the distinctively developing cultures in the two countries were fundamentally shaped by similar relationships to a single, central cultural core, in London. This common marginality—a similar distance from and involvement with the same central core—was a shaping element in the growth of each of these provincial cultures and was necessary to explain both. We tried, rather too ambitiously, to draw out the implications of this observation in an essay entitled “England’s Cultural Provinces: Scotland and America.”²⁷ We were convinced that the formulation was correct, but we did not then realize the magnitude of the issues. We did not know how our literary data related to an overall British Atlantic social system or what other kinds of events and documentation might be seen to be involved in this system. Indeed, we did not know what kind of a system, one small corner of which we were examining, this really was.

At about the same time David Quinn began publishing some unusually suggestive studies of England’s overseas expansion and settlement in the sixteenth century. In them he noted, first, that many of those who were involved in settlements in Ireland were also involved in settlements in America; and, second, that the attitude of the English to natives encountered in these two colonial areas was remarkably similar, and that experience gained in one area was automatically applied in the other.²⁸ From Quinn’s writing alone one began to see that the origins of England’s overseas empire were part of something more comprehensive, which included the British Isles themselves as well as overseas territories. What was involved was an expansion of the English, later British, world from its core in southeastern England out into a series of expanding alien peripheries—Wales and the North Country of England in the sixteenth century, Scotland, Ireland, and North America in the seventeenth century. Phrases linking various British overseas territories, scarcely noticed before, suddenly took on heavy meaning: Ireland was described in a travel book of 1617, for example, as “this famous island in the *Virginian* sea.”²⁹ One could envision a huge, outwardly expanding peripheral arc sweeping north and west from London and the Home Counties into Wales and Lowland Scotland, across Ireland, southwest through Newfoundland, then down the North American coast through Nova Scotia, New England, the Chesapeake, and the Carolinas, and ending in the many Anglo-American settlements in the Caribbean. This arc was

²⁷ John Clive and Bernard Bailyn, “England’s Cultural Provinces: Scotland and America,” *William and Mary Quarterly*, 3d ser., 11 (1954): 200–13.

²⁸ Quinn, *Raleigh and the British Empire* (London, 1947), esp. chap. 5, and *The Elizabethans and the Irish* (Ithaca, N.Y., 1966). For later developments of this theme, see Nicholas P. Canny, “The Ideology of English Colonization: From Ireland to America,” *William and Mary Quarterly*, 3d ser., 30 (1973): 575–98, and “The Permissive Frontier: The Problem of Social Control in English Settlements in Ireland and Virginia, 1550–1650,” in K. R. Andrews et al., eds., *The Westward Enterprise: English Activities in Ireland, the Atlantic, and America, 1480–1650* (Liverpool, 1978); James Muldoon, “The Indian as Irishman,” *Essex Institute Historical Collections*, 111 (1975): 267–89.

²⁹ Fynes Moryson, *An Itinerary* (London, 1617), as quoted in Quinn, *The Elizabethans and the Irish*, 122 (italics added).

nothing so simple as the trade route of an empire in the traditional sense, commercial or territorial. Nor was it merely an expanding frontier line. It was not a line, an edge, comprehensible in Turnerian terms as such, but a ring of territories, of marchlands, separated in important ways from the territories on either side of it.³⁰ In these linked territories a central culture encountered a variety of different human and physical environments and formed a variety of new subcultures, all of which were contained within a single overall system that might be designated "British." But, even broadened out to all of these magnitudes, one's vision proved to be too restricted. It remained for J. G. A. Pocock, a New Zealander educated in England and long resident in the United States, to suggest that this entire interactive Atlantic culture system, this huge band of variant marchlands, was in itself only a segment of a global system that ultimately reached Southeast Asia, Australia, New Zealand, and other parts of the Pacific world as well.³¹

The ramifications of such a view—applicable to far more than the British world or to the other world empires—are extensive and important. Issues arising in various locations within the periphery, which once seemed disparate and discrete, can now be seen to be closely related, and the relationships help explain the course of events. In this perspective, for example, it becomes apparent that official British policy, promulgated in London, restraining the settlement of the trans-Appalachian west in America was shaped in part by attitudes to Scotland and Ireland—the fear of Scottish and absentee Irish landlords in high office in London that their lands would be depopulated by the extension of settlement in America and, hence, that the economic stability of their lives would be threatened as Americans migrated west into areas four thousand miles from Whitehall.³² One suddenly understands the reach and penetration of Dr. Johnson's imagination when he observed, on his tour of the western Scottish islands in 1773, that the attraction of the American frontier to discontented Highlanders on the Scottish frontier was a threat to the survival of British culture. Highlanders relocated on the far western British periphery, he said, will simply be lost to the nation: "For a nation scattered in the boundless regions of America resembles rays diverging from a focus. All the rays remain but the heat is gone. Their power consisted in their concentration: when they are dispersed, they have no effect."³³ Was such a dispersal outward from the center to the margins, with its attendant loss of "concentration," wise? Could it be stopped? Could British law be used to prevent the circulation of British people along the peripheries of British territory? What should be the proper relationships of the outer boundaries to each other and to the core? These problems, which take on meaning only insofar as one grasps not just the eighteenth-century American

³⁰ Denys Hay made this point precisely in his essay on the Scottish marchlands, "England, Scotland, and Europe: The Problem of the Frontier," *Transactions of the Royal Historical Society*, 5th ser., 25 (1975): 77–91, esp. 80.

³¹ Pocock, "British History: A Plea for a New Subject," *Journal of Modern History*, 47 (1975): 601–21. Cf. Michael Hechter, *Internal Colonialism: The Celtic Fringe in British National Development, 1536–1966* (London, 1975), and the exchange of views between Pocock and Hechter following Pocock's article, pp. 626–28.

³² Bailyn, "1776: A Year of Challenge—A World Transformed," *Journal of Law and Economics*, 19 (1976): 458–59.

³³ Samuel Johnson, *A Journey to the Western Islands of Scotland*, ed. Mary Lascelles (New Haven, 1971), 94–99, 131.

frontier but the British world system in its entirety, were being discussed actively at the highest level of the British government in November and December 1773 and were at the point of resolution in a controversial proposal that Parliament prohibit further British migration to America, when the conflict between Britain and the colonies put an end to the discussion.³⁴

Migration and the problem of the imperial constitution are two aspects of the general issue of core-periphery relations in the early modern British world; there are others. Political institutions and political ideas whose origins lay in the heartland took on different forms in the differing peripheral settings. It was the peculiar impact of American circumstances on political forms and ideas emanating from the metropolitan culture of Britain that determined the shape of public institutions in the United States.³⁵ But this pan-Atlantic British system of the early modern period cannot be understood in isolation from certain other large systems of the time. Essential to it are intersections with other systems moving discretely within their own patterns.

An explanation of the population history of British North America in the preindustrial period also involves the depiction of a Central European system concentrated in the upper Rhineland but spreading out northeast to the Danish border, east to Bohemia, and southeast through the Danubian basin to southern Russia. Spin-offs from that distinctive and independently evolving system, whose major flows were eastward into Prussia, the Habsburg lands, and Russia, entered directly into the British galaxy of the eighteenth century as the first of some seventy-five thousand "Germans" (in fact, Swiss and French Protestants from the region of Montbéliard as well as subjects of the German princes) began moving down the Rhine, transshipping at Rotterdam and Cowes to reach the *Insel*, as it was sometimes called in the Rhineland, of *Bintzel-vannier* (Pennsylvania).³⁶ Not only can one plot the intersection of the central European population system with the British, but

³⁴ Rumors circulated continuously in 1773 that parliamentary legislation was being drafted to prohibit all further emigration to America, and in November 1773 two London newspapers (*Lloyd's Evening Post*, November 15–17, and the *Public Advertiser*, November 16) finally carried the supposed text of a radical seven-point plan "to be proposed at the next meeting of Parliament to prevent the emigration of our people to America." Issued, no doubt, as Franklin surmised "to feel the pulse of the public," it elicited from Franklin a carefully reasoned reply, which he apparently never published, and touched off heated public debates not only in the metropolitan areas of Britain but in some of the provinces as well. [Franklin] *The Papers of Benjamin Franklin*, 20, ed. William B. Wilcox et al. (New Haven, 1976): 522–28. Although a prohibitory bill was never enacted, actions of other kinds were undertaken. Some of the most powerful men in Ireland (including Franklin's *bête noir*, the former secretary of state for the colonies, Lord Hillsborough), who were supporting prohibitory legislation, formed an association to lower rents where they were felt to be oppressive. And in Scotland, the Lord Justice Clerk, Thomas Miller, began a parish-by-parish survey of the magnitude of the exodus, an inquiry that was eventually extended by the British government to the registration of every British subject who left Britain from December 1773 to March 1776. The subject is discussed in general and a statistical analysis of the resulting register of English and Scottish emigrants is presented in my forthcoming *Voyagers to the West*.

³⁵ Bailyn, *Ideological Origins of the American Revolution* (Cambridge, Mass., 1967), esp. chaps. 5, 6; Gordon S. Wood, *Creation of the American Republic, 1776–1787* (Chapel Hill, 1969), pt. 2.

³⁶ Otto Langguth, "Pennsylvania German Pioneers from the County of Wertheim," [*Yearbook of*] *The Pennsylvania German Folklore Society*, 12 (1947): 169–70. There is no comprehensive account of the Central European population movements. For a brief sketch of these flows as they relate to the migration to Pennsylvania and for references to a few of the many German writings on the subject, see Marianne Wokeck, "The Flow and Composition of German Immigration to Philadelphia, 1727–1775," *Pennsylvania Magazine of History and Biography*, 105 (1981): 274–78.

one can identify individuals whose role it was to forge links between the two independently moving orbits. Benjamin Furly, William Penn's friend and agent, long resident in Rotterdam—merchant, intellectual, land developer, and defender of liberal causes—was the first of these key figures. But the intersections were not limited to Europe; they involved West Africa as well. For the West African population system, too, spilled over into segments of the British Atlantic world, which was spreading deep into the Ohio and Mississippi Valleys, along the Florida coasts, and within the maritime provinces of Canada.³⁷ To see the whole of the entire set of interrelated systems that impinged on preindustrial America one would have to circle the globe like a satellite and note the simultaneous movement of peoples and cultures across a vast area—an area stretching from the Elbe to the Mississippi and from the North Sea to the Congo.

Such a synoptic view develops most readily from the study of population movements. But the concept of inclusive systems with centers and margins, whose integrity as systems is essential to understanding the individual parts within them, is applicable in many spheres. The heart of the transformation of the Roman state and society that Syme narrated in *The Roman Revolution* lay in the reconstitution of the governing class by recruitment from the provinces. "The strength and vitality of an empire," Syme wrote, "is frequently due to the new aristocracy from the periphery." From Roman Spain, he explained, local notables

migrate to the capital in permanence; they purchase mansions at Rome, villas and estates in the fashionable vicinity; they invade the high strata of society; they contract marriage alliances with Italian families, and even with the old Roman aristocracy; and also, and naturally, with similar groups of rising families from other provinces. . . . They began as clients of the Caesars and they end by supplanting them.

And, in an interesting sketch, Syme discussed the failure of this recruitment, reinforcement, and freshening from the overseas peripheries in the case of the Spanish and the first British empires.³⁸

A similarly synoptic view has proved effective in intellectual history as well, most notably in two series of distinguished publications. The first is Franco Venturi's sensitive description of the radiations of the Enlightenment from its center in Paris to the near peripheries in Western Europe—Spain, Italy, Corsica, Austria, Germany, and England—and then to the outer margins in Eastern Europe, Russia, and North America. With his exceptional linguistic ability and his broad vision, Venturi has been able to show not merely the general penetration of reform ideas into the remote provinces of the Western world but also the specific adaptations of these ideas that were made in different cultures. His elaborate tracing of the circulation of Beccaria's *On Crimes and Punishments* from its origins in Milan through the whole of Europe shows the possibility of this kind of study. The second is J. G. A. Pocock's elaborate tracing of a single body of political thought—the peculiar language and grammar of "civic humanism"—from Florence to England, Scotland, and America.

³⁷ William I. Hull, *Benjamin Furly and Quakerism in Rotterdam* (Swarthmore, Pa., 1941), and *William Penn and the Dutch Quaker Migration to Pennsylvania* (Swarthmore, Pa., 1935), esp. 328–45; Philip D. Curtin, *Economic Change in Precolonial Africa: Senegambia in the Era of the Slave Trade* (Madison, Wis., 1975), chaps. 2–4.

³⁸ Syme, *Colonial Elites: Rome, Spain, and the Americas* (London, 1958), 4, 13.

"A 'language' is uncovered in sixteenth-century Florence," Pocock wrote in a recent summary, "and shown becoming first Puritan, then Whig, then American" as it circulated "away from Europe, towards what is least European in the Anglophone (or 'Atlantic') world."³⁹

In a different vein, closer to the approach of François Furet and his collaborators in the collective inquiry *Livre et Société*, is Robert Darnton's book on the publishing history and distribution of the *Encyclopédie*. Through an exhaustive examination of the marketing of the quarto edition of the *Encyclopédie*, Darnton traced the distribution of this key work of the Enlightenment—and, hence, to a significant degree the diffusion of the Enlightenment itself—from the center in Paris to the French provinces and then out to the Low Countries, the Rhineland, "the north European plains to the Scandinavian fiords and the Russian steppes until finally it reached remote outposts like Lex's bookshop in Warsaw and Rüdiger's in Moscow." Through Darnton's eyes we can picture volumes being "hauled across the snow from Leipzig [to St. Petersburg] by sled," and moving up the Elbe and the Moldau, across the Alps to Turin, down the Rhône to Marseilles and Genoa, and along the Danube to Pest, "where," Darnton wrote, "Paris seemed centuries away in contrast to the immediacy of the Ottoman Empire and the unremitting warfare on the eastern front of western culture."⁴⁰ A similarly comprehensive view enabled Robert Palmer and, to a lesser extent, Jacques Godechot to grasp as a singular concatenation of events the pan-European and American explosions of "democratic revolutions" of the late eighteenth century. The possibilities have been shown to be rich in other spheres as well—in analyzing the history of domestic politics (notably American Populism) and a wide range of contemporary phenomena: international relations, political geography, the value systems of organized society, urban environments, and the dissemination of art forms, both fine and applied. And other orbits can be envisioned in other connections: news dissemination, technical expertise, literary forms, business practices.⁴¹

THUS, IT SEEMS TO ME, IN THE WELTER of historical publications, there are not only signs of a deepening interpenetration of latent and manifest events but also the outlines of systems of filiation and derivation among phenomena that once were

³⁹ Much of the argument of Venturi's three-volume *Settecento riformatore* (Turin, 1969–79), which traces the spread of Enlightenment ideas throughout the Western world, is summarized in his *Utopia and Reform in the Enlightenment* (Cambridge, 1971) and in his "Church and Reform in Enlightenment Italy: The Sixties of the Eighteenth Century," *Journal of Modern History*, 48 (1976): 215–32. He began his tracking of the circulation of Beccaria's ideas in a report to an international conference on Beccaria convened by the Academy of Sciences of Turin in 1964 (the proceedings of which were published by the academy in 1966) and brought his work together in an edition of Beccaria's treatise, which reprints the work itself in 104 pages and then presents as a 547-page appendix a documentary history of the origins of the book in Milan and its reception in Italy, France, England, Spain, Switzerland, Austria, Germany, Denmark, Sweden, and Russia. Cesare Beccaria, *Dei delitti e delle pene* (1764), ed. Franco Venturi (Turin, 1965; 3d edn., 1973); for a summary, see Venturi, *Utopia and Reform in the Enlightenment*, chap. 4. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975), and "The Machiavellian Moment Revisited: A Study in History and Ideology," *Journal of Modern History*, 53 (1981): 71.

⁴⁰ Darnton, *The Business of Enlightenment: A Publishing History of the Encyclopédie, 1775–1800* (Cambridge, Mass., 1979), 530, chaps. 6, 10, apps. B, C, and "The *Encyclopédie* Wars of Prerevolutionary France," *AHR*, 78 (1973): 1346–52. Cf. François Furet et al., *Livre et société dans la France du XVIII^e siècle*, 2 vols. (Paris, 1965–70).

⁴¹ R. R. Palmer, *The Age of the Democratic Revolution*, 2 vols. (Princeton, 1959–64); and Godechot, *La*

discussed in isolation from each other. And, third, there is also in motion in current historical writing an intensifying effort to relate the world of interior, subjective experiences to the course of external events.

Long before it became fashionable to talk about the study of *mentalité*, and well before William Langer had challenged historians to take as their next assignment the application of psychoanalytic principles to historical problems,⁴² historians had attempted to describe the state of people's awareness. They had sought to depict, however crudely, not only people's ideas and beliefs as expressed in formal discourse but their deeper, interior life: the assumptions, attitudes, fears, expectations, and aspirations that together formed people's private construction of the world, their personal map of reality, their system of ordering life, of imposing meaning on the stream of experience. But it has always been extremely difficult to probe the strange interior worlds of the past, partly because the historian has no means of inquiring directly into the condition of people's awareness, partly because in the end historians are more interested in communities of people than in unique individuals. The characterization of a community's interior life, even when its members stand alive before one, available for interviewing, polling, and participant observation, is problematic for the anthropologists, sociologists, and psychologists who design methods precisely for such studies. For historians, lacking living subjects and dependent on random documentation, all of the difficulties are compounded.

Occasionally there has been a Huizinga capable of painting a more or less convincing picture of a great transition in a society's perception of the world by an impressionistic study of art forms and by imaginative projection into the likely experiences of everyday life. And there have been books like Oscar Handlin's *The Uprooted* (1951) that trace, through empathy and intuition as well as through documentation, the inner lives of generations of people adjusting to new environments. But most such efforts turn into a vague literary impressionism that reveals as much about the author as about the past, or into a study of formal texts that are

Grande Nation: L'Expansion révolutionnaire de la France dans le monde, de 1789 à 1799, 2 vols. (Paris, 1956). On Populism as a movement "on the fringe of the metropolitan culture," see James Turner, "Understanding the Populists," *Journal of American History*, 67 (1980–81): esp. 370–71. For twelve wide-ranging essays applying this concept, covering political geography in general, America's place in the world, the regionalism of social change in Italy, nation-building in southeastern Europe, newly independent island clusters, bilingualism in Montreal, a "geoethnic-geoeconomic-geopolitical model of differentiation within western Europe," and the splintering of states in Eurasia, see Jean Gottmann, ed., *Centre and Periphery: Spatial Variations in Politics* (Beverly Hills, 1980). On value systems and social organization, see Edward Shils, "Centre and Periphery," in *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi on His Seventieth Birthday, 11th March 1961* (London [1961]), 117–30. For an attempt to use this concept to explain "capitalist agriculture and the origins of the European world-economy in the sixteenth century," see Immanuel Wallerstein, *The Modern World-System, I* (New York, 1974); in the second volume (New York, 1980), the same scheme is used in an effort to explain "mercantilism and the consolidation of the European world-economy, 1600–1750." For orbits of cultural dissemination, see, for example, Milton Newton, "Cultural Preadaptation and the Upland South," in H. J. Walker and W. G. Haag, eds., *Man and Cultural Heritage*, volume 5 of *Geoscience and Man* (Baton Rouge, La., 1974), 143–54; Robert D. Mitchell, "The Formation of Early American Culture Regions: An Interpretation," in James R. Gibson, ed., *European Settlement and Development in North America: Essays on Geographical Change in Honour and Memory of Andrew Hill Clark* (Toronto, 1978), 66–90; C. Lee Hopple, "Spatial Development of the Southeastern Pennsylvania Plain Dutch Community to 1970," *Pennsylvania Folklife*, 21 (1971–72), no. 2: 14–40, and no. 3: 36–45; E. Estyn Evans, "The Scotch-Irish: Their Cultural Adaptation and Heritage in the American Old West," in E. R. R. Green, ed., *Essays in Scotch-Irish History* (New York, 1969), 69–86.

⁴² William L. Langer, "The Next Assignment," *AHR*, 63 (1957–58): 283–304.

supposed somehow to add up to a picture of the “mind of the Middle Ages,” *l’esprit laïque*, or *l’esprit bourgeois*. Even in what would seem to be the most manageable aspect of the problem—in the biographies of key historical figures whose individual actions unquestionably shaped events and about whom a great deal is known—the difficulties of exploring interior worlds of subjective experience are great. In any case, collective biography is most often the main question for historians, and to probe beyond what people did, wrote, and said to what they experienced, how they felt, and how they comprehended the world remains a major challenge to historical investigation.

In certain areas historical scholarship has shown great progress in recent years in reaching into subjective experience. While technical psychohistory is still more a matter of theoretical discussion by social scientists than of scholarly practice by historians, ways have been found to explore public opinion in the past, attitudes of various kinds, and the pervasiveness and circulation of certain key notions. The range of such studies has been broad. Political thought has provided an important entrée. Working out from the strict genealogy of ideas to the broader aspects of political thought where ideas connect with more general social assumptions and attitudes, historians have been able to enter private worlds otherwise closed to them. So Gordon Schochet’s *Patriarchalism* is ostensibly a study in “political thought,” but, in fact, it relates a key concept in political thought to deep-lying social attitudes shared as interior experiences by whole populations in the seventeenth century. W. H. Greenleaf’s *Order, Empiricism, and Politics* is also explicitly a study in political thought; but in fact it explores certain presumptions concerning the nature of reality in the broadest sense, the “great hinterland” of beliefs, attitudes, ideas, and assumptions experienced by whole populations. So too, in different ways, do the books and articles of a whole squadron of writers on political “ideology” involved in the American, French, and Russian Revolutions.⁴³

And other, even more original and imaginative ways have been found to enter the realm of interior experience. Some of the most interesting have reached into nonverbal expressions of private experience and established subtle connections between nonverbal and verbal communication. Carl Schorske’s *Fin-de-Siècle Vienna*, in which aspects of interior worlds are uncovered through examination of the connections among a variety of expressions of art forms, has set an attractive new style in scholarship. Schorske’s deliberate fusions of urban architecture and political attitudes, of painting and “the liberal ego,” and of the descriptive and metaphoric meanings of the garden—these connections among art forms and public life, constructed into a general picture of a community’s “psyche,” are already being emulated and seem destined to shape the work of many historians of culture seeking a deeper understanding of human experience than traditional historical analysis provides. Schorske’s style was, in fact, influential even before his book

⁴³ Gordon J. Schochet, *Patriarchalism in Political Thought* (New York, 1975), esp. chaps. 3, 4; Greenleaf, *Order, Empiricism, and Politics: Two Traditions of English Political Thought, 1500–1700* (London, 1964). For a review of such writings on American themes, see Robert E. Shalhope, “Toward a Republican Synthesis: The Emergence of an Understanding of Republicanism in American Historiography,” *William and Mary Quarterly*, 3d ser., 29 (1972): 49–80.

appeared. Six years earlier his student William McGrath published *Dionysian Art and Populist Politics in Austria*, which not only demonstrates the common pan-Germanistic roots of both Viktor Adler's socialism and Gustav Mahler's music and "meta-musical cosmos" (passages from the score of Mahler's Third Symphony precede a chapter on the Liberals' Linz Program) but locates the exact origins of all of these diverging lines of history in the shared outlook, the common interior world, of a particular circle of students in the 1870s, a circle that first formed in a single secondary school, Vienna's *Schottengymnasium*, and then in a political club at the University of Vienna.⁴⁴ These writings on the German-speaking world of the late nineteenth and early twentieth centuries, writings that are beginning to form a genre of their own, may one day be brought into useful comparisons with accounts of similar circles in other cultures: Bloomsbury, Yeats' Dublin, or Herzen's world of Russian exiles in London, for example, circles with distinctive and highly articulated sensibilities, attitudes, and world views. And, indeed, it may be possible to depict the cultural history of an entire era in terms of key "circles" of shared feelings and outlooks.

Studies like Schorske's *Vienna* and McGrath's *Dionysian Art* have concentrated on art forms in probing perceptions of the world, orderings of reality. But the perceptions and orderings that they depict are those of highly cultivated individuals whose relation to ordinary experience may be remote. Efforts have also been made to compose pictures of the inner experiences of less cultivated people—to map the private worlds of ordinary people. Recent studies in popular culture based on nonverbal, behavioral expressions have been revealing—studies like those of Natalie Davis on the festivals of misrule in sixteenth-century France, of Rhys Isaac on the political theatre of eighteenth-century Virginia, and of John Brewer on popular mock elections in Georgian England, a work whose main sources are satirical prints.⁴⁵ But the most extreme and impressive examples are found in two areas. The first is in nineteenth-century French history: in Theodore Zeldin's extraordinary account of "the common beliefs, attitudes and values of Frenchmen," their "unspoken assumptions," their "ambitions, human relationships and the forces which influenced thinking"; and in Guy Thuillier's exploration of the color, sound, taste, pace, and tactile feel of the life of ordinary people in Nevers—the *invisible quotidien* of existence, seen in the use of water, personal hygiene, the pattern of rising and retiring, the "archaeology of gestures," all of which he drew from a great mass of documents buried accidentally, like tiny chips of stone, in the vast landscape of the past. The second area lies in the exploration of religious sensibilities in the widest and subtlest sense, ranging from Norman Cohn's *Pursuit of the Millennium*, on medieval chiliastic movements, and Perry Miller's volumes on the anatomy of the New England mind, to the remarkable studies by Keith Thomas

⁴⁴ Carl E. Schorske, *Fin-de-Siècle Vienna: Politics and Culture* (New York, 1980); William J. McGrath, *Dionysian Art and Populist Politics in Austria* (New Haven, 1974).

⁴⁵ Natalie Zemon Davis, *Society and Culture in Early Modern France* (Stanford, 1975), chap. 4; Isaac, "Dramatizing the Ideology of Revolution: Popular Mobilization in Virginia, 1774–1776," *William and Mary Quarterly*, 3d ser., 33 (1976): 357–85; Brewer, "Theatre and Counter-Theatre in Georgian Politics: The Mock Election at Garrat," *Radical History Review*, 22 (1979–80): 7–40.

and Alan Macfarlane on the psychology and sociology of witchcraft and magic in early modern England. These are pathbreaking books, rich and carefully nuanced.⁴⁶

At the level, then, simply of the depiction of interior worlds—patterns of attitudes, beliefs, fears, and aspirations that together organize people’s engagement with the exterior world—progress has been made, and there is no question, it seems to me, that we will see much more of this kind of history, ranging from further studies in political ideology to an expanded cartography of the *invisible quotidien* and of religious sensibilities. But in the end the question historians must answer is the relation of these interior worlds to the exterior world of palpable historical events. How is this area of private history, reflecting interior states of awareness, to be related to the external course of events in the past, events of a public nature? To leave these private worlds isolated from the public—to keep the internal separated from the external and to ignore the problem of the effects of the one upon the other—is to evade the central obligation of history, which is to describe how and explain why the course of events took the path it did.

There is no issue of principle here. Obviously what people did was related to what they carried about in their heads: their feelings, their attitudes, their construction of reality. This is obvious in studying individuals, but in studying “peoples” the question skitters off into “climates of opinion” vaguely, if at all, related to the determination of specific events. The problem is inescapable, however, and more and more, in the years ahead, historians will seek answers. They will, that is, seek connections between interior world views—shared attitudes and responses and “mind-sets”—and the course of external events. But, as responses to recent forays into this terrain at the rather obvious level of exploring the “ideological origins” of certain major political events have indicated, establishing the relation of outward events to the submerged world of private awareness is difficult and bound to be controversial.

THUS, WITHIN THE GREAT MASS OF CONTEMPORARY HISTORIOGRAPHY there are, it seems to me, at least three general trends in motion, three lines of development generated by the force of scholarship itself, which will in varying ways enrich, but also complicate, any comprehensive narratives that are written: the fusion of latent and manifest events; the depiction of large-scale spheres and systems organized as peripheries and cores; and the description of internal states of mind and their relation to external circumstances and events. None of this, of course, is wholly new. Each has anticipations and early formulations. The Marxists have always struggled to construe history as the manifestation of latent events. Toynbee’s construction of history, within his leading notion of challenge and response, is that of central and marginal orbits of world civilizations. And not only did Burckhardt in his

⁴⁶ Zeldin, *France, 1848–1945*, 2 vols. (Oxford, 1973–77), 1: 2, 8; Thuillier, *Pour une histoire du quotidien au XIX^e siècle en Nivernais* (Paris, 1977); Cohn, *The Pursuit of the Millennium* (New York, 1957); Miller, *The New England Mind: The Seventeenth Century* (Cambridge, Mass., 1939), and *The New England Mind: From Colony to Province* (Cambridge, Mass., 1953); Thomas, *Religion and the Decline of Magic* (London, 1971); Macfarlane, *Witchcraft in Tudor and Stuart England* (London, 1970). See also Emmanuel LeRoy Ladurie, *Montaillou, village occitan de 1294 à 1324* (Paris, 1976), translated into English by Barbara Bray (London, 1978).

Civilization of the Renaissance in Italy (1860) examine world views, attitudes, and intellectual styles, but a century ago Karl Lamprecht, once a fiercely controversial figure and now largely forgotten, advocated a historiography explicitly and “scientifically” concentrated on collective psychology and internal states of awareness. Lamprecht’s search for the “*Seelenleben*, the psychic life, psychic activity, psychic state” of the German *Volk* led him into studies of individual as well as collective consciousness and of external artifacts of all kinds as expressions of subjective experience.⁴⁷ But these anticipations of the present ferment in history were either isolated, programmatic, or metahistorical, or they were caught up in heady delusions about history becoming a “science”—a notion that has persisted, in varying forms, from Lamprecht’s time and before through the New Historians of the early twentieth century to the more enthusiastic *Annales* scholars of our own time, to receive what one hopes will be its terminal apotheosis at the hands of our colleague, Robert Fogel.⁴⁸

What distinguishes the present developments I have sketched is that they are substantive, not methodological or merely exhortative. Further, the works involved are not isolated probes by uniquely imaginative individuals but the cumulating work of many scholars, most of whom are unaware that they are contributing to a general development. And, above all, they are rich enough in content to bear directly on the fulfillment, at a new level of sophistication, of the ultimate purpose of all historical scholarship, comprehensive narration. The greatest challenge that

⁴⁷ Toynbee’s sweeping perspective gave him range to perceive the existence of such orbits long before there was evidence with which to describe them accurately; his writing on this theme is, therefore, at times quite fanciful. See, for example, his discussion of “Scotland—Ulster—Appalachia,” in *A Study of History*, 2 (London, 1934): 309–13, which is in general a perceptive sketch of the British marchlands of the early modern period but which contains such wonderful passages as the following: “the Appalachian ‘Mountain People’ at this day are no better than barbarians. They are the American counterparts of the latter-day White barbarians of the Old World: the Rifis and Kabyles and Tuareg, the Albanians and Caucasians, the Kurds and the Pathans and the Hairy Ainu.” But, despite such analogies, Toynbee well understood the relation of the domestic British “*Völkerwanderung*” and interior marches to the North American frontier. He drew some remarkable insights from a book now long forgotten that anticipated with surprising clarity the ideas of Quinn, Canny, and Muldoon: William C. Macleod, *The American Indian Frontier* (London, 1928), chap. 13: “Celt and Indian: Britain’s Old World Frontier in Relation to the New.” Toynbee, *A Study of History*, 1 (London, 1934): Annex, pp. 465–67. Three decades later, Toynbee enthusiastically introduced a new edition of Walter Prescott Webb’s *The Great Frontier* (Austin, 1964), a book in which Webb, magnifying Frederick Jackson Turner’s interpretation of American history to global dimensions, wrote of the interplay between the Great Frontier in the colonial territories and the Metropolis in Europe as “the drama of western history.” For Quinn, Canny, and Muldoon, see note 28, above.

On Lamprecht, see Karl J. Weintraub, *Visions of Culture* (Chicago, 1966), 167, chap. 4; and Annie M. Popper, “Karl Lamprecht,” in Bernadotte Schmitt, ed., *Some Historians of Modern Europe* (Chicago, 1942), 217–39. Lamprecht’s ideas, which created a storm in Germany, were rejected there and his prodigious efforts (including his twenty-one-volume *Deutsche Geschichte*) written off as a tissue of errors, hopelessly schematic and methodologically unsound. But he was honored by historians in the United States, who found—in the broadly based psychogenetic *Kulturgeschichte* he advocated and wrote—elements of the reform program that would become known as the New History. (The name itself seems to have originated in a favorable review essay: E. W. Dow, “Features of the New History: Apropos of Lamprecht’s ‘Deutsche Geschichte,’” *AHR*, 3 [1897–98]: 431–48.) Carl Becker was particularly intrigued and puzzled by Lamprecht’s ideas. They seemed to support his interest in climates of opinion but yet to verge on sheer fancy. Lamprecht’s concentration on the “one common underlying psychic mechanism” in the histories of nations and cultures, Becker wrote, threatened to transform the real substance of history into “social experience deposited in nerve centers.” Becker, “Some Aspects of the Influence of Social Problems and Ideas upon the Study and Writing of History,” *American Journal of Sociology*, 18 (1913): 673–74.

⁴⁸ Robert W. Fogel, “‘Scientific’ History and Traditional History,” in L. J. Cohen et al., eds., *Logic, Methodology, and the Philosophy of Science*, 6 (Amsterdam, 1980).

will face historians in the years ahead, it seems to me, is not how to deepen and further sophisticate their technical probes of life in the past (that effort will, and of course should, continue in any case) but how to put the story together again, now with a complexity and an analytic dimension never envisioned before; how to draw together the information available (quantitative and qualitative, statistical and literary, visual and oral) into readable accounts of major developments. These narratives will incorporate anecdote but they will not be essentially anecdotal; they will include static, "motionless" portrayals of situations, circumstances, and points of view of the past, but they will be essentially dynamic; they will concentrate on change, transition, and the passage of time; and they will show how major aspects of the present world were shaped—acquired their character—in the process of their emergence. No effective historian of the future can be innocent of statistics, and indeed he or she should probably be a literate amateur economist, psychologist, anthropologist, sociologist, and geographer. In the end, however, historians must be, not analysts of isolated technical problems abstracted from the past, but narrators of worlds in motion—worlds as complex, unpredictable, and transient as our own. The historian must re-tell, with a new richness, the story of what some one of the worlds of the past was, how it ceased to be what it was, how it faded and blended into new configurations, how at every stage what was, was the product of what had been, and developed into what no one could have anticipated—all of this to help us understand how we came to be the way we are, and to extend the poor reach of our own immediate experience.

Thucydides and Spartan Strategy in the Archidamian War

THOMAS KELLY

THE ARCHIDAMIAN WAR, which ended with the Peace of Nicias in 421 B.C., is frequently viewed as a classic example of a war between a land power and a sea power. When this first phase of the Peloponnesian War began in 431 B.C., the Athenian navy was the largest and finest in the Greek world; for decades it had dominated the waters of the eastern Mediterranean. The Athenian army, however, was no match for the Spartan army, which for more than a century had been recognized as the most efficient fighting force in Greece. The stage was set, then, for Athenian victories at sea and Spartan domination on land.

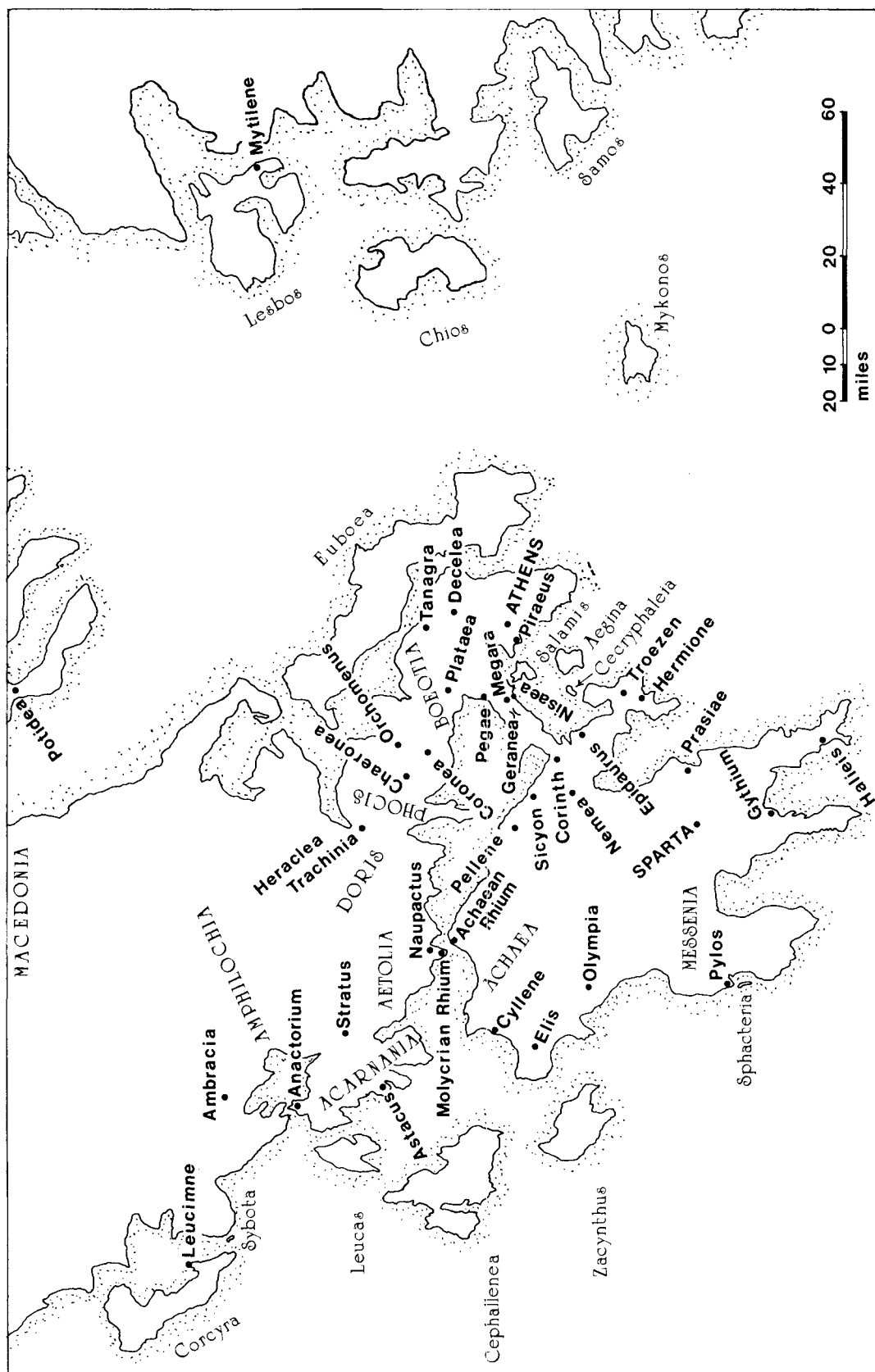
At several places in his *History of the Peloponnesian War*, Thucydides assures us that in 431 B.C. the Spartans believed they could force a swift end to the Peloponnesian War by ravaging Athenian territory. In one passage he tells us that they believed such a strategy would result in victory in a few years; in another passage, where he does not mention the Spartans by name, he tells us that some Greeks believed the Athenians could withstand such invasions for one year, others for two years, but that no one believed they could hold out for more than three years.¹ Elsewhere, however, he informs us that at the outset of the war the Spartans sent orders to friendly states in Sicily and Italy to supply money and ships for the war effort, and with such aid the Spartans hoped to raise a fleet of five hundred ships.² There is an obvious incongruity here, for, if the Spartans really believed that invasion of Attica would end the war in a few years, they had no need to build up their naval forces. It is a tribute to our stereotyped notions of both Athens and Sparta—due in no small part to Thucydides himself—that students of the Peloponnesian War have long disregarded or sought to explain away the report that the Spartans hoped to build up their naval forces, while accepting unequivocally the report that invasion of Attica alone was expected to bring the war to a successful conclusion.

Such selective use of Thucydides is unjustified. If we are to understand Sparta's strategy for carrying on the war with Athens, we cannot arbitrarily reject one passage because it does not fit our preconceived notions and accept another passage because it does. This has, nonetheless, been the *modus operandi* of modern scholars,

Many of the ideas expressed here were originally given in a paper presented at the annual meeting of the American Association of Ancient Historians, held in Seattle, May 1–4, 1979. I have profited much from the formal commentary delivered on that occasion by Professor Raphael Sealey. I am also grateful to Professor Richard C. Nelson for drawing the map.

¹ Thucydides 5.14.3, 7.28.3. In addition to these two passages, also see Thucydides 1.81.1–2, 4.85.2, where invasion of Attica is alluded to in Thucydidean speeches.

² Thucydides 2.7.2.



who, secure in their belief that Spartan strategy was fully understood, regarded the subject as unworthy of independent investigation. Not until 1965 was Spartan strategy first treated in detail. In that year P. A. Brunt carefully laid out the enormous difficulties and disadvantages Sparta and its allies would have encountered in attempting to compete with the Athenians on the sea.³ Not only was the Peloponnesian fleet outnumbered, but a shortage of trained crewmen and money prevented them from putting to sea all of the ships in their possession. When they did sail out, they confronted an enemy superior in tactical skill and self-confidence. Faced with these difficulties, the Spartans simply could not take the offensive on the sea, and that, in any case, was not their aim. The Spartans entered the war, Brunt has argued, with the experience of their earlier war with Athens—the so-called First Peloponnesian War—uppermost in their minds. In 446 the Spartans had invaded Attica, and this invasion was quickly followed by the Thirty Years Truce in which the Athenians made important concessions. It was in view of this experience that in 431 the Spartans believed, as Thucydides informs us, that invasion of Attica would lead to speedy resolution of the conflict.⁴

Although this proposition was by no means original with Brunt, he was the first to assemble the evidence in support of it, and an impressive array of evidence it is. We know, for example, that when the Samians revolted and tried to secede from the Athenian empire in 440, the Spartans wanted to come to their aid, presumably by invading Attica, and in the winter of 433–32 the “authorities” at Sparta promised to invade Attica to assist the Potideans in their confrontation with Athens.⁵ On the surface, at least, there would seem to be little doubt that invasion of Attica remained the keystone of Spartan strategy for waging any war against Athens, and Brunt’s treatment of the subject has now come to be regarded as definitive.⁶

It cannot, of course, be denied that there were Spartans who believed that an invasion of Attica would quickly bring the Athenians to their knees, but it does not follow that all Spartans shared this belief. Sparta was clearly not the monolithic society it is often made out to be, and there is absolutely no reason to suppose that there were not differences of opinion there. Neither war nor peace is a one-dimensional issue, and, if there was to be peace with Athens in 431, surely Spartan opinion differed as to how high a price ought to be paid. Alternately, if there was to be war, surely there were differences of opinion as to what the war ought to accomplish for Sparta and, above all, differences of opinion as to how it would have to be waged.

In a recent re-examination of Spartan strategy in the Archidamian War, George L. Cawkwell has rightly recognized that such differences of opinion did exist.⁷

³ Brunt, “Spartan Policy and Strategy in the Archidamian War,” *Phoenix*, 19 (1965): 255–80. I do not cite specific portions of Brunt’s article here.

⁴ Thucydides 5.14.3.

⁵ For Samos, see Thucydides 1.40.5, 1.41.2; both references are in a speech of the Corinthians. For Potidaea, see Thucydides 1.58.1. On the date of the Samian revolt, see Charles W. Fornara, “On the Chronology of the Samian War,” *Journal of Hellenic Studies*, 99 (1979): 7–19.

⁶ See, for example, Donald Kagan, *The Archidamian War* (Ithaca, 1974), 18–20; Geoffrey E. M. de Ste. Croix, *The Origins of the Peloponnesian War* (Ithaca, 1972), 210; and Paul A. Cartledge, *Sparta and Lakonia: A Regional History* (London, 1979), 234–35. The discussion of Spartan strategy in each of these works is based on Brunt’s article.

⁷ Cawkwell, “Thucydides’ Judgment of Periclean Strategy,” *Yale Classical Studies*, 24 (1976): 53–69.

There were more radical Spartans who wished to see Athens crushed and the Greeks liberated, while others of more moderate persuasion were willing to treat with the Athenians. These differences of opinion gave rise to different military strategies; in fact, Cawkwell has argued, there was not a single strategy followed at Sparta during the war but two different strategies employed simultaneously. These he has labeled the “conventional” and the “adventurous.” The “conventional” strategy, supported by moderate Spartans, consisted of ravaging Attica in the hope that this would bring on a decisive battle or force the enemy to submit. The “adventurous” strategy, supported by the more radical element, called for the war to be carried on in other ways—for example, by promoting revolt among Athens’ allies and by building up Peloponnesian naval forces. Only after the capture of the Spartans at Sphacteria in 425, he has argued, was the “adventurous” strategy pursued in earnest. Until that time, it played some part in the conduct of the war; but, basically, the Spartans sought to win by the conventional means of invading Attica, and this strategy seemed to be effective.

Although Cawkwell is surely correct in recognizing that not all Spartans were of the same mind with respect to the Athenians and that there were differences of opinion as to how the war would have to be waged, Spartan military activity in the Archidamian War cannot be neatly separated into the “conventional” and “adventurous” categories he has proposed. A single example will suffice: in 427 the Spartans dispatched a fleet to Lesbos, an action that Cawkwell specifically included among his examples of the “adventurous” strategy in operation. At the same time as this fleet was being dispatched, however, Peloponnesian land forces invaded Attica, an action that can only be seen as an example of the “conventional” strategy in operation. Thucydides informs us, however, that the situation was more complex than this, for the invasion of Attica was intended solely to occupy the Athenians and divert their attention from the fleet as it crossed the Aegean.⁸ In this instance, at least, the “adventurous” dispatch of a fleet and the “conventional” invasion of Attica were employed side by side and in concert. Consequently, neither operation can be viewed in isolation, for each formed part of a larger, over-all strategy.

If we are to understand this strategy we cannot be selective in our use of Thucydides. Nor can we employ as our primary evidence general statements of belief or intent that are found in his work. Instead, we must examine in their proper context Spartan military and diplomatic initiatives as related by Thucydides. Before this examination can be undertaken, however, preliminary matters must be taken up, especially the First Peloponnesian War, any lessons the Spartans may have learned from it, and, of equal importance, the information known and available to the Spartans on the eve of the war in 431 B.C.

THE SPARTANS MAY WELL HAVE ENTERED the Archidamian War with the experience of the First Peloponnesian War uppermost in their minds, but wars rarely provide clear-cut, unambiguous lessons. The First Peloponnesian War was no exception, and, if some Spartans concluded from it that Athens could be brought to its knees

⁸ Thucydides 3.26.1.

by an invasion, other Spartans may easily have learned different lessons. Indeed, it does not strain the evidence to say that they *should* have learned other lessons. Although the invasion of Attica and the Thirty Years Truce of 446 are closely related, they are not so closely connected that all else can be ignored. In truth, it was not solely the invasion but a variety of other problems that contributed to Athenian willingness to make concessions in the Thirty Years Truce.

Russell Meiggs has argued forcefully that the early years of the decade of the 440s, and especially the year 447, was a time of troubles for the Athenians.⁹ Disaffection was widespread, though uncoordinated, throughout the empire. Although resolved by 446, many of these difficulties would have been fresh in the Athenian mind as events of this year unfolded and problems shifted closer to home. In Boeotia, Chaeronea and Orchomenos were lost; Tolmides quickly recovered Chaeronea, but, before Orchomenos could be regained, he was killed and his army defeated at Coronea. In order to recover the prisoners taken there, the Athenians were forced to evacuate all of Boeotia. These reverses encouraged the Euboeans to revolt. Pericles was dispatched to subdue the rebels, but hardly had he landed on the island when news reached Athens that the garrison at Megara had been massacred. The defection of Megara provided the Spartans with a direct invasion route to Attica, and they were quick to seize the opportunity. To meet the threat of invasion, Pericles had to be recalled from Euboea. Shortly thereafter, both sides agreed to the Thirty Years Truce.¹⁰ If some Spartans attributed the truce to the invasion alone, others must have grasped that a whole series of reverses suffered in quick succession had led the Athenians to yield.

Moreover, anyone who reflected seriously on the events of the war could hardly fail to recognize that Sparta and its allies were especially vulnerable to the superior naval might of the Athenians.¹¹ Even a cursory consideration of several instances in which the Peloponnesians suffered at the hands of Athenian naval power suggests that this should have been the outstanding lesson to be learned from the war. The Athenians were victorious in the only naval engagements that took place; at Cecryphaleia over the Peloponnesian fleet and at Aegina where seventy Aeginetan vessels were captured.¹² Even land operations were affected by Athenian naval presence. In 458 a force of 1,500 Spartan heavy infantry and 10,000 allies moved across the Gulf of Corinth to aid the Dorians of Doris in their struggle against the Phocians. This force was apparently transported by sea, thus demonstrating that Sparta and its allies had sufficient naval forces to convey a sizeable army beyond the Peloponnesus. Subsequent events show, however, that they had little confidence in these forces. The Phocians were quickly defeated; but the Spartans feared to return to the Peloponnesus by sea, and with the Athenians in control of Geranea they did not wish to risk returning by land either. The army was forced to winter north of

⁹ Meiggs, *The Athenian Empire* (Oxford, 1972), 152–75.

¹⁰ For these events, see Thucydides 1.113.1–1.115.1; and Diodorus 16.6.1–2. Also see Ste. Croix, *Origins of the Peloponnesian War*, 196–200.

¹¹ For full accounts of the war, see Donald Kagan, *The Outbreak of the Peloponnesian War* (Ithaca, 1969), 77–130; and Edouard Will, *Le Monde grec et l'orient* (Paris, 1972), 149–70. On Sparta's role in the war, see Ste. Croix, *Origins of the Peloponnesian War*, 181–200; and A. J. Halladay, "Sparta's Role in the First Peloponnesian War," *Journal of Hellenic Studies*, 97 (1977): 54–63. Halladay has argued that the Spartans were cautious in going to war against a state whose strength differed so radically from their own.

¹² On these naval battles, see Thucydides 1.105.1–2; and Diodorus 11.70.2.

the Isthmus. Not until it had defeated an army of Athens and its allies at Tanagra the following year did the Spartan expedition feel confident enough to return home over land. After its withdrawal the Athenians quickly marched into Boeotia, reasserted control there, and made substantial gains in Phocis as well. The Spartans were either unwilling or unable to move forces into this area a second time and made no effort to counter these moves.¹³

Twice during the war Athenian fleets circumnavigated the Peloponnesus, and on each occasion achieved notable success. A fleet commanded by Tolmides set out from the Piraeus and for a brief time gained control of Laconian Methone and Gythium, where the Spartan dockyards were burned. Zacynthus and Cephallenia were taken, as was Naupactus, where the Messenian rebels were settled, and Sicyonian territory was ravaged.¹⁴ Several years later a fleet commanded by Pericles set out from Pegae and circumnavigated the Peloponnesus in the opposite direction. On this expedition the Athenians were able to gain allies in Achaea, to ravage parts of Acarnania, and on one occasion to advance into the interior of the Peloponnesus as far as Nemea, where a Sicyonian force was defeated. According to Plutarch, these exploits made Pericles celebrated even among foreigners, and we should expect them to have the same impact at Sparta. Sometime during the war Athenian forces occupied Troezen, and before it came to an end Aegina was forced into total submission, its walls demolished and its fleet taken over.¹⁵ Henceforth, this previously staunch supporter of Sparta was a tribute-paying member of the Athenian empire.

With memories such as these it would be surprising indeed if everyone at Sparta was convinced in 431 that invasion of Attica would quickly resolve the upcoming war, as Thucydides assures us; and, in any case, this assurance is called into question by the information he supplies in a passage so important that it deserves to be quoted in full:

Now that the affair at Plataea had occurred and the treaty had been glaringly violated, the Athenians began preparing for war, and the Lacedaemonians also began; both sides were making ready to send embassies to the King and to the barbarians of any other land, where either of them hoped to secure aid, and they were negotiating alliances with such cities as were outside their own sphere of influence. The Lacedaemonians, on their part, gave orders to those in Italy and Sicily who had chosen their side to build, in proportion to the size of their cities, other ships, in addition to those which were already in Peloponnesian ports, their hope being that their fleet would reach a grand total of five hundred ships, and to provide a stated sum of money; but as to other matters, they were instructed

¹³ Thucydides 1.107.2–1.108.3. Thucydides does not say specifically here that Peloponnesian forces moved into Phocis by sea, but Ste. Croix has forcefully argued that they did; *Origins of the Peloponnesian War*, 190–95. T. T. B. Ryder has offered a translation of Thucydides 1.107.3 that has the Athenians moving a fleet into the Gulf of Corinth to confine the Spartans in central Greece; Ryder, “Thucydides and Athenian Strategy in the Early 450’s: A Consensus of Mistranslations,” *Greece and Rome*, 25 (1978): 121–24.

¹⁴ On Tolmides’ circumnavigation, see Thucydides 1.108.5; and Diodorus 11.11.84. On the settlement of the Messenians at Naupactus, see Thucydides 1.103.3; and Diodorus 11.84.7–8. For further discussion of the events treated in this paragraph, also see Norbert Brockmeyer, “Athens maritime Strategie gegenüber dem Peloponnesischen Bund von Themistokles bis Perikles,” *Der altsprachliche Unterricht*, 14 (1971): 37–63.

¹⁵ On Pericles, see Thucydides 1.111.2–3; Diodorus 11.85.1–2, 11.88.1–2; and Plutarch *Pericles* 19.2–3. For the occupation of Troezen, see Thucydides 1.115.1. For the submission of Aegina, see Thucydides 1.108.4; and, on the role of Aegina in the First Peloponnesian War, see Thomas J. Figueira, “Aeginetan Membership in the Peloponnesian League,” *Classical Philology*, 76 (1981): 1–24, esp. 19–23.

to remain inactive and to refuse their ports to Athenians if they came with more than a single ship, until these preparations had been completed.¹⁶

One of the major stumbling blocks to accepting the information contained here is the number of ships Thucydides says the Spartans hoped to acquire. Although the number five hundred is consistent in all manuscripts, it has, as Gomme and many others have noted, an impossible ring. There is, however, no justification for dismissing it out of hand or emending it on the basis of information supplied by Diodorus.¹⁷ For the moment it will be useful to disregard this precise figure and consider the other information contained in the passage.

What we learn is that in one of their first acts after the Theban attack on Plataea the Spartans sent orders to friendly states in Italy and Sicily to build ships and provide money; at the same time embassies were being prepared for dispatch to the king of Persia and to other barbarians. With aid from these quarters the Spartans hoped to increase significantly the size of their fleet. Here in a very few words Thucydides provides us with clear and unequivocal proof that, whatever else the Spartans may have been planning in 431, they were also contemplating building up their naval forces. Until valid reasons for rejecting this information are offered, we have no choice but to accept it and, of equal importance, accept its implications. If, in 431, the Spartans were undertaking measures to build up Peloponnesian naval forces, they were obviously not convinced that invasion of Attica alone would end the war and that it would, therefore, be necessary to conduct operations at sea as well as on land. These are the salient points of the passage, and they are of far greater import for our understanding of Spartan strategy than whether or not the number "five hundred" is accurate. Even if it is dismissed as erroneous, the other information contained in the passage and its clear implications remain unaffected and must be accepted.

IT WOULD BE NAIVE TO INSIST on the absolute integrity of the figure, but any assessment of its accuracy ought to begin not with an arbitrary rejection but, rather, with a consideration of the information known and available to the Spartans in 431. Although the Spartans had no sizeable navy of their own, they did have access, through their allies, to substantially larger naval forces than is generally admitted. Thucydides tells us specifically¹⁸ that no fewer than seven cities—Corinth, Sicyon,

¹⁶ Thucydides 2.7.1–2. For this and all other direct quotations from Thucydides, I have relied on the translation of Charles F. Smith, *Thucydides*, Loeb Classical Library (London, 1919).

¹⁷ Arnold W. Gomme, in commenting on this passage, has called five hundred ships "an impossible number"; Gomme, *A Historical Commentary on Thucydides*, 5 vols. (Oxford, 1945–80), 2: 7. Gomme's view is shared by most scholars. Diodorus says that the Spartans persuaded their allies in Sicily and Italy to come to their aid with two hundred triremes; Diodorus 12.41.1.

¹⁸ Thucydides 2.9.3. Kagan has set the size of the Peloponnesian fleet at "about 100"; *Archidamian War*, 21. Others have placed it a bit higher, at 150 ships; see, for example, B. W. Henderson, *The Great War between Athens and Sparta* (London, 1925), 83. Nicholas G. L. Hammond has credited Sparta and its allies with a fleet of 300 ships; Hammond, *A History of Greece* (2d edn., Oxford, 1967), 311–12. But, on this figure, see the observations of Ste. Croix, *Origins of the Peloponnesian War*, 67 n. 12. I have more fully examined the naval strength of Sparta and its allies elsewhere; see my "Peloponnesian Naval Strength and Sparta's Plans for Waging War against Athens in 431 B.C.," in *Studies in Honor of Tom B. Jones: Alter Orient und Altes Testament*, no. 203 (1979): 245–55, where additional bibliography can be found.

Megara, Pellene, Elis, Ambracia, and Leucas—contributed to the Peloponnesian fleet. He does not tell us how many ships each of these states possessed nor does he tell us the size of the combined fleet. We do know, however, that five of these states—Corinth, Megara, Elis, Ambracia, and Leucas—accounted for 149 of the 150 ship fleet that sailed against the Corcyreans in 433. Of these, 30 were lost in the engagement at Sybota, but, so far as we know, all but 1 of the 120 ships that survived the battle were still available for duty two years later when the Archidamian War broke out. Moreover, at least one state, Megara, was by then in a position to provide a considerably larger number of ships, 40, than the 12 it had contributed in 433.¹⁹ Unfortunately, we have no information on the number of ships available to Sicyon or Pellene; nor to the Spartans, who certainly had a few of their own. It is also possible that smaller allies such as Epidaurus, Hermione, and Troezen were able to make at least nominal additions to the fleet as they had done four years earlier, when jointly they had sent 8 ships with the Corinthian fleet that engaged the Corcyreans off Leucimne.²⁰ In view of these numbers it does not seem at all unreasonable to postulate a combined Peloponnesian fleet numbering approximately 150 vessels at minimum, and the total may well have been considerably higher. At the very least we know that in the summer of 430 a Peloponnesian fleet of 100 ships put to sea, and this fleet did not include 40 Megarian ships then in drydock at Nisaea.²¹ Admittedly, a fleet of 150 ships was certainly no match for the much larger Athenian fleet, which numbered 300, but if Sparta hoped to raise a fleet of 500 ships it was certainly a good beginning, a nucleus to which additional ships could be added.

One of the key factors not always taken into account in attempting to understand Spartan hopes at the beginning of the war is the dramatic build-up in Peloponnesian naval strength in the years between 435 and 433 B.C. In the middle of the decade Corinth and its allies had sent a fleet of 75 ships against the Corcyreans at Leucimne. Only 40 ships survived that engagement, but immediately thereafter the Corinthians began a crash building program that quickly produced substantial results. When the combined Corinthian-allied fleet put to sea two years later, it consisted of 150 ships, nearly a four-fold increase.²² There can be no doubt that this

¹⁹ On the number of ships contributed by each state, see Thucydides 1.46.1; and, on the number lost, see Thucydides 1.54.2. Only one state, Anactorium, is listed as contributing to the Corinthian fleet in 433 but not to the Peloponnesian fleet in 431, and its contribution was but a single trireme. Note, however, that Nicholas G. L. Hammond has argued that some of the 120 ships that survived the battle were disabled beyond seaworthiness; Hammond, "Naval Operations off Corcyra," in his *Studies in Greek History* (Oxford, 1973), 466–68. On the number of Megarian ships, see Thucydides 1.46.1, 2.93.2.

²⁰ Thucydides remarks that the Spartans preyed on merchant shipping along the Peloponnesian coast, so they must have had some ships of their own; Thucydides 2.67.4. Later in the war, these three states, along with Megara, were ordered to build a combined total of ten ships; Thucydides 8.3.2.

²¹ On the Peloponnesian fleet in 430, see Thucydides 2.66.1, 2.93.2. The latter passage proves that the fleet did not at that time include the ships in drydock at Nisaea. For especially useful comments on the latter passage, see Gomme, *A Historical Commentary on Thucydides*, 2: 237–39.

²² On the size of the fleet in 435, see Thucydides 1.27.2; on the losses suffered at Leucimne, see Thucydides 1.29.5; and, on the size of the fleet in 433, see Thucydides 1.46.1. Only fifteen Corinthian ships survived the battle at Leucimne, but in 433 the Corinthians had ninety ships. Although Thucydides speaks of feverish building activity specifically only at Corinth, the Ambraciot contingent went from eight ships in 435 to twenty-seven in 433. Ronald P. Legon has properly labeled this rapid growth in Peloponnesian naval strength as "among the most significant military developments of the fifth century"; Legon, "The Megarian Decree and the Balance of Greek Naval Power," *Classical Philology*, 68 (1973): 169.

sharp growth in Peloponnesian naval strength was cause for genuine concern at Athens. Thucydides does not say so specifically, but he certainly implies²³ that this concern was one of the reasons why the Athenians acceded to Corcyrean appeals for an alliance in 433.

The ability of the Peloponnesians to put together from their own resources such a large fleet in so short a time was not the only encouraging sign on the horizon. The Aeginetans were urging the Spartans to declare war on Athens,²⁴ and, if this strategically placed island could be detached from the empire, the Peloponnesians would have a naval base at the very gates of the Piraeus. Overtures for an alliance had already come from the Lesbians,²⁵ and their naval forces were considerable. Overtures had also come from Macedon, and, although Perdiccas was not a man to be trusted, he certainly had no love for Athens and did have money and timber, both so desperately needed by Sparta and its allies.²⁶ At Corcyra the Corinthians had skillfully laid a plan they hoped would rupture the alliance with Athens and lead to the formation of an alliance between the Corcyreans and the Peloponnesians. Next to the Athenians, the Corcyreans had the largest fleet among the Greek states, 120 triremes before their losses to the Corinthians at Sybota.²⁷ In the east, the same Persian king, Artaxerxes, who had provided the Spartans with money during the First Peloponnesian War, was still ruling.²⁸ In the west, the Doric states of Sicily except Camarina allied with the Spartans as the war began, and there was no reason to believe that they would not contribute to the war effort.²⁹

All of this information was available to the Spartans in 431, and it is in the light of this information that their hope of raising a fleet of five hundred ships must be viewed. The picture contains a great many "ifs," to be sure. But, if ships and money

²³ Thucydides says that the Athenians "did not wish to give up Corcyra, which had so large a fleet, to the Corinthians, but hoped to wear them out upon one another as much as possible, in order that the Corinthians as well as other naval powers might be found weaker in case they had to go to war with them"; Thucydides (Loeb edn.) 1.44.2–5. Also see Ste. Croix, *Origins of the Peloponnesian War*, 75. In addition to understanding the military importance of sea power, the Athenians appreciated its political uses as well; Edward N. Luttwak, *The Political Uses of Sea Power* (Baltimore, 1979), 71–72.

²⁴ Thucydides 1.67.2, 2.27.1.

²⁵ Thucydides 3.2.1, 3.13.1. On the date of the Lesbian overtures, see the discussion in Gomme, *A Historical Commentary on Thucydides*, 2: 252.

²⁶ Thucydides 1.67.4. Thucydides also states here that Perdiccas kept sending envoys to Sparta in an effort to bring about a war between Athens and Sparta. Later in the war, Perdiccas financed Spartan operations in the north; Thucydides 4.83.6. Also see Thucydides 4.80.1. For confirmation of Athenian concern for Macedonian timber, see *IG*, 1², 71. The dating of the inscription is insecure, but the date must fall somewhere between 437 and 422 B.C.; Hermann Bengtson, *Die Staatsverträge des Altertums* (Munich, 1962), Nr. 186. Richard J. Hoffman has argued that the treaty recorded on the stone dates from the summer of 431; Hoffman, "Perdikkas and the Outbreak of the Peloponnesian War," *Greek, Roman, and Byzantine Studies*, 16 (1975): 359–77.

²⁷ Thucydides 1.55.1, 3.70.1. On the size of the Corcyrean fleet, see Thucydides 1.25.4.

²⁸ Thucydides says that Artaxerxes' envoy, Megabazus, believed the king's money was being spent in vain, although Diodorus says it was not accepted by the Spartans; Thucydides 1.109.1–2; and Diodorus 11.74.5–6. And, for a discussion of this issue, see David M. Lewis, *Sparta and Persia* (Leiden, 1977), 62–63. On the interest of the Persian King in Greek affairs, see Alfred T. Olmstead, *History of the Persian Empire* (Chicago, 1948), 346–48; and Samuel K. Eddy, "The Cold War between Athens and Persia, ca. 448–412 B.C.," *Classical Philology*, 68 (1973): 253–54.

²⁹ On the western allies of Sparta, see Thucydides 3.86.2. The possibility of Spartan aid from the western Greeks is frequently discounted; see, for example, Brunt, "Spartan Policy and Strategy in the Archidamian War," 262. But, as H. D. Westlake has pointed out, the Athenians certainly could not discount this possibility; Westlake, "Athenian Aims in Sicily, 427–424 B.C.," in his *Essays on Greek Historians and Greek History* (Manchester, 1969), 113–16. Also see Kagan, *Archidamian War*, 183–84.

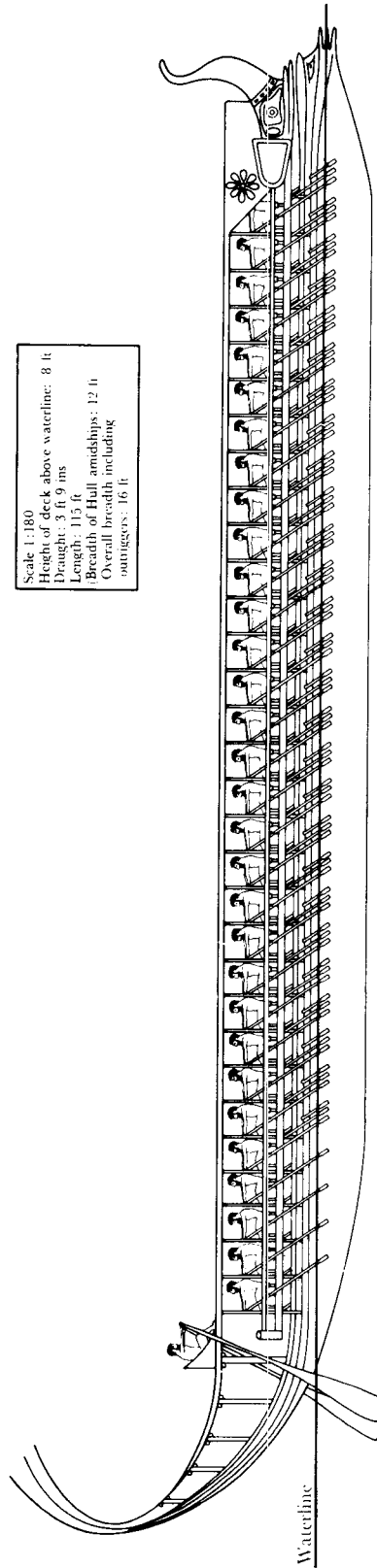


Figure 1: Profile view of a Greek trireme.

Line drawing reproduced courtesy of Cambridge University Press from J. S. Morrison and R. T. Williams, *Greek Oared Ships* (Cambridge, 1968), [plate 31.]

were supplied by friendly states in Sicily and Italy, if aid came from the Persians, if Aegina could be taken over, if Perdiccas could be enlisted as an ally, if Lesbos should rebel against Athens, if Corcyra should come over to the Peloponnesian side, and if a crash building program could be undertaken, then raising a fleet of five hundred ships was certainly possible. That all of these conditions would be fulfilled was unlikely. But, in view of Thucydides' statement that the Greeks were favorably inclined toward the Spartans, it was equally unlikely that none of them would. With the aid of hindsight, we know that this is precisely what happened, but the only thing this proves is that the Spartans seriously miscalculated the willingness of their fellow Greeks and the Persians to come to their aid. It does not justify dismissing Thucydides' plain statement that they hoped to acquire a fleet of five hundred ships, a figure that, for what it may be worth, receives vague support from Diodorus.³⁰ Finally, if there was any sincerity in the Spartan claim that they were going to war for the liberation of the Greeks,³¹ they must have known that this could only be accomplished with naval forces. On balance, to believe that the Spartans misjudged the situation seems just as reasonable as to believe that Thucydides made a mistake in his reporting. He was not, to be sure, infallible, but neither were the Spartans. Thucydides provides evidence of other Spartan miscalculations, and one of these occurred in an area particularly pertinent to the present discussion.

The Spartans would have encountered, as Brunt has pointed out, many drawbacks in attempting to wage naval warfare against the Athenians.³² Even if the money necessary to finance the construction of ships, hire crews to man them, and defray the cost of conducting naval operations had become available to the Spartans, they would have had a far more difficult time facing the Athenians on the sea than they initially anticipated. They seriously underestimated the value of Athenian experience in naval warfare and the handicap under which they themselves would be operating as a result. Although it is an exaggeration to say that Spartan interest in the sea was wholly nonexistent before the war,³³ the Spartans had never paid close attention to naval matters. Raphael Sealey has recently placed the depth of their inexperience in perspective; he has shown that the nauarch was

³⁰ Diodorus says the Spartans persuaded their allies in Italy and Sicily to come to their aid with 200 triremes; Diodorus 12.41.1. Also see note 17, above. If this number were added to the number of ships already available to the Spartans, Thucydides' figure of 500 becomes more reasonable.

³¹ Thucydides 1.140.3, 2.8.4.

³² Brunt, "Spartan Policy and Strategy in the Archidamian War," 259–61. Also see Eduard Meyer, *Geschichte des Altertums* (Stuttgart and Basel, 1956), 4, 26–28.

³³ For the evidence, see Luigi Pareti, "Ricerche sulle potenze marittime degli Spartani e sulla cronologia dei navarchi," *Memorie dell'Accademia delle Scienze di Torino*, 2d ser., 59 (1908–09): 71–159, reprinted in *Studi minori di storia antica*, 2 (Rome, 1961): esp. 1–14. Werner Peek has published an inscription recording a treaty between Sparta and the otherwise unknown Erxadieis of Aetolia; Peek, "Ein neuer spartanischer Staatsvertrag," *Abhandlungen der sächsischen Akademie der Wissenschaft zu Leipzig, Philologische-historische Klasse*, 65 (1975): 1–15. The treaty is a clear indication of Sparta's willingness to enter an alliance with a state lying beyond the Peloponnesus. In one of the provisions of the treaty, the Erxadieis agreed to follow the Spartans "wheresoever they shall lead by land and by sea" (κατὰ γᾶν καὶ καθάλαθρον), but this may be merely formulaic; see Arnaldo Momigliano, "Terra marique," *Journal of Roman Studies*, 32 (1942): 62–64. Peek's date for the inscription is the early decades of the fifth century ("500–470?"). This date is accepted by Fritz Gschnitzer, but Paul A. Cartledge has suggested a date in the 420s; Gschnitzer, *Ein neuer spartanischer Staatsvertrag und die Verfassung des peloponnesischen Bundes* (Meissenheim a./G., 1978), 34; and Cartledge, "A New 5th Century Spartan Treaty," *Liverpool Classical Monthly*, 1 (1976): 87–92. This lower dating would fit well with the arguments advanced here, but it is too uncertain to be pressed.

not even an annual official at Sparta during the Archidamian War.³⁴ Yet the Spartans optimistically believed they would have no trouble gaining the naval experience necessary to compete with the Athenians. One incident reported by Thucydides makes this abundantly clear. In 429, when news of Phormio's brilliant victory over a much larger Corinthian fleet reached Sparta, it was greeted with rage and utter disbelief that inexperience could have had anything to do with the defeat. The Spartans blamed it, rather, on *malakía*, a lack of energy or vigor.³⁵ Obviously, they had not yet learned to appreciate fully the value of Athens' greater expertise in seamanship, and, if in the third year of the war they could still be guilty of such self-deception, they could easily have misjudged the problems involved in raising a fleet of five hundred ships.

No discussion of Spartan strategy in the Archidamian War would be complete without some mention of the speeches in Book I of Thucydides' *History*. If we could be at all sure that the ideas contained in them reflect the views of the speakers rather than the views of Thucydides, we would have irrefutable proof that talk of Peloponnesian naval activity against the Athenians was very much in the air shortly before the outbreak of the war. This theme plays a substantial role in the speeches of the Corinthians, of Pericles, and of Archidamus.³⁶ The Corinthians, in setting forth the reasons why the Peloponnesians will emerge victorious, include their superiority in land warfare, their unity and obedience to a single command, and their ability to equip and man a fleet as well as gain the experience to compete with the Athenians in naval warfare. They speak also of encouraging Athenian subjects to revolt, of planting forts in Athenian territory, and, more vaguely, of "other measures which one cannot now foresee." Many of the points put forth by the Corinthians are countered in the speech of Pericles, but the speech of Archidamus is especially important. He expresses the belief that invasion of Attica alone will not win the war and that the only way the Spartans can hope to emerge victorious is either by gaining mastery of the sea or cutting off Athenian revenue. Neither seemed very likely to Archidamus, who, like the Corinthians, acknowledged Athenian superiority in ships, naval experience, and financial resources. Unlike the Corinthians, however, Archidamus was not at all confident that Peloponnesian deficiencies in these areas could be easily overcome, and he advocated avoiding war

³⁴ Sealey, "Die spartanische Nauarchie," *Klio*, 58 (1976): 335–58.

³⁵ Thucydides 2.85.1–2. On the meaning of *malakía*, see Gomme, *A Historical Commentary on Thucydides*, 2: 220. That the Spartans were sincere in this belief is clear from their hurried dispatch of three advisers—Timocrates, Brasidas, and Lycophron—to provide the energy and vigor that was lacking under Cnemus.

³⁶ For the Corinthian speech, see Thucydides 1.120–24; for Pericles' speech, see Thucydides 1.140–44; and, for Archidamus's speech, see Thucydides 1.80–85. For a fuller discussion of the naval implications of these speeches, see my "Peloponnesian Naval Strength," 249–50. And, for an examination of their importance for understanding Spartan strategy, see Cawkwell, "Thucydides' Judgment of Periclean Strategy," 65–68; and Ian Moxon, "Thucydides' Account of Spartan Strategy and Policy in the Archidamian War," *Rivista Storica dell' Antichità*, 8 (1978): 7–26. I am less convinced than most that the speeches provide us with valid evidence for assessing Spartan plans in 431; and, in the case of Archidamus in particular, I think it highly unlikely that such a speech was ever delivered by the Spartan king. It bears noting, however, that the sentiments expressed in the speech are clearly not those of a pacifist, as Archidamus is sometimes made to appear. Elie Bar-Hen has pointed out that, although Archidamus does express a willingness to arbitrate, he does not speak against war with Athens—only against a declaration of war before adequate preparations had been made; Bar-Hen, "Le Parti de la paix à Sparte à la veille de la guerre du Péloponnèse," *Ancient Society*, 8 (1977): 21–31.

until adequate preparations had been made, particularly with respect to the fleet and financial reserves.

Too many of the ideas expressed in these speeches smack of composition in the light of later events to inspire much confidence that they were actually delivered on the eve of the war. The allusion in the speeches of the Corinthians and Pericles to fortifying Athenian territory might well have been written with the occupation of Decelea in mind, and Archidamus' admonition that the war could not be won by invading Attica is inconsistent with Thucydides' report that he expected the invasion of 431 to produce results.³⁷ Although the speeches may be helpful, our understanding of Spartan strategy ought not rest on them but, rather, on the narrative of Thucydides. And to this we must now turn.

WHEN HOSTILITIES BEGAN IN THE SPRING of 431, the Spartans had neither sufficient ships nor adequate naval experience to challenge Athens on the sea. Unless and until aid could be obtained from Sicily, Italy, or Persia, their only recourse was to invade Attica. The Athenians, on the advice of Pericles, remained behind their fortification walls and refused to engage the Spartan army. Although Pericles' policy of abandoning Attica to the enemy gave rise to serious discontent, his will prevailed.³⁸ The Spartans were apparently taken by surprise; even Archidamus was sure that the Athenians would not stand idly by and allow their fields to be ravaged. When, in the first few weeks of the war, it became apparent that they were resolved to do so, the Spartans, if they had not recognized it before, must now have realized that any strategy based solely upon invading Attica could lead only to a lengthy war of attrition.³⁹

The second Peloponnesian military operation in this first year of the war must be viewed with these events in mind. In the spring, the Athenians sent out a fleet of one hundred triremes, reinforced by fifty Corcyrean ships. After ravaging the Peloponnesian coast, the Athenians conducted operations in Acarnania, where they expelled Euarchus from Astacus and took this city into their alliance. In the winter of 431–30 Euarchus persuaded the Corinthians to assist him in regaining control of Astacus. To this end they dispatched a force of 1,500 hoplites and forty ships, and Euarchus was successfully restored to power. Before returning home, the Corinthi-

³⁷ Thucydides has Archidamus express his expectation in a speech, and he twice relates the same information in his narrative; Thucydides 2.11.6–7 and 2.18.5, 2.20.1–3. In both narrative passages Thucydides employs the term λέγεται ("it is said"), and he may not, therefore, have been absolutely sure of the accuracy of the report; on this point, see H. D. Westlake, "ΛΕΓΕΤΑΙ in Thucydides," *Mnemosyne*, 30 (1977): 352–54. Also see Jacqueline de Romilly, "Les Intentions d'Archidamos et le livre II de Thucydide," *Revue des études anciennes*, 64 (1962): 287–99.

³⁸ For the invasion, see Thucydides 2.18–23. According to Thucydides, the first place the Peloponnesian forces attacked in Attica was Oeonoe, a fortress on the Boeotian-Athenian border, and, when they retreated, they withdrew via Boeotia; Thucydides 2.18.1–3, 2.23.3. On the basis of these passages, W. P. Wallace has argued that Archidamus must have transported his army across the Gulf of Corinth by sea and returned to the Peloponnese the same way; Wallace, "The Spartan Invasion of Attica in 431 B.C.," in Mary E. White, ed., *Studies in Honor of Gilbert Norwood* (Toronto, 1952), 80–84.

³⁹ To assume that the Spartans learned nothing from this series of events is to underestimate seriously Spartan intelligence. C. A. Powell has argued that Spartan foreign and military policy toward Athens was more profound and intelligently applied than is commonly recognized; Powell, "Athens' Difficulty, Sparta's Opportunities: Causation and the Peloponnesian War," *L'Antiquité classique*, 49 (1980): 87–114.

ans attacked other coastal areas of Acarnania as well as Cephallenia, but without success.⁴⁰ Little has been made of this expedition, but it is not likely that the Corinthians were acting on their own. Although Thucydides makes no mention of the Spartans in conjunction with the operation, it is difficult to believe that they did not at least tacitly approve. In any case, the first Peloponnesian military initiative of the war was an invasion of Attica, but the second was a naval expedition to Acarnania. In the very first year of the war the Peloponnesians had served notice that they were able to conduct operations outside of Attica and, more to the point, were willing to employ their naval forces to do so.

This did not occasion any surprise at Athens. Indeed, a number of Athenian military initiatives in 431 were designed to make it as difficult as possible for Sparta and its allies to wage war at sea. Immediately after the Theban attack on Plataea, the Athenians prepared to dispatch embassies to the Persians and other barbarians; ambassadors were actually sent to Corcyra, Cephallenia, Zacynthus, and Acarnania.⁴¹ According to Thucydides, the Athenians believed that if they could maintain friendly relations with these states they might be able to encircle and subdue the Peloponnesus. It was essential that Corcyra with its large fleet and strategic location not fall into enemy hands. Neither Cephallenia nor Zacynthus nor Acarnania had any substantial naval resources,⁴² but their location, individually and collectively, was strategically even more important than that of Corcyra. They were situated between Leucas and Ambracia, two major contributors to the Peloponnesian fleet, on the one hand, and Corinth, Sicyon, and Elis, also major contributors to the fleet, on the other. So long as these states remained friendly, the Athenians could count on being kept informed of any movement of enemy ships in these waters. More importantly, the two islands, Cephallenia and Zacynthus, would provide the Athenians with secure bases from which their fleet could operate to seal off the Gulf of Corinth and thereby prevent Corinthian and Sicyonian ships from joining up with Leucadian and Ambraciot ships. As events turned out, the Athenians used Naupactus for this purpose during the war, but at the outset they quite possibly saw greater security in island bases rather than Naupactus, which was within easy reach of Spartan land forces.

Athenian interest in Corcyra, Cephallenia, Zacynthus, and Acarnania in 431 suggests that they did not discount the possibility of enemy naval operations in the war, and this suggestion receives strong reinforcement from other Athenian actions in this year. Immediately after Archidamus withdrew his forces from Attica, the Athenians posted guards by land and sea. The assembly voted to establish a reserve fund of 1,000 talents and a reserve force of one hundred triremes to be used only in the event of an enemy attack by sea. The seriousness with which they regarded the possibility of such an attack is apparent from the imposition of the death penalty for anyone proposing to use either the money or the ships for any other

⁴⁰ On the Athenian fleet, see Thucydides 2.23.2–3, 2.25.1–5. On the expulsion of Euarchus, see Thucydides 2.31.1; and, on Corinthian participation in his restoration, see Thucydides 2.33.1–3.

⁴¹ Thucydides 2.7.1, 2.7.3.

⁴² Cephallenia, however, may have been viewed as a potential trouble spot by the Athenians. Although the Cephallenians had not aided the Corinthians in 433, four ships from Cephallenia were part of the Corinthian fleet that sailed against Corcyra in 435; Thucydides 1.47.2.

purpose. (In fact, only after the disaster at Syracuse were the funds finally expended in 412.)⁴³ We do not know if the embassy being prepared for Persia actually set out in this year, but we do know that steps were hurriedly taken to bring Perdiccas back into line and to convert Sitalces, a former enemy, into a friend. With equal rapidity Aegina was occupied and its population displaced, for strategic reasons, as Thucydides clearly implies, and efforts were made to insure the safety of Euboea. A fleet consisting of one hundred Athenian and fifty allied vessels was sent to ravage the Peloponnesian coast,⁴⁴ and the very size of this fleet may well indicate that the Athenians were not at all certain they could attack the enemy by sea without opposition.

Clearly, these are not the actions of a state convinced that enemy military activity would be confined to invading Attica, and this should come as no surprise. The Athenians had no way of knowing that the aid so desperately needed by Sparta and its allies would not be forthcoming, and they could not operate on the assumption that it would not. Publicly, Pericles might scoff at Sparta's ability to compete with the Athenians on the sea, but privately the possibility had to be given serious consideration. If the Spartans had hopes, the Athenians had fears. Indeed, Thucydides, in a speech that he puts into the mouth of Phormio as he awaited attack by a much larger Peloponnesian force in the summer of 429, states the case in precisely these terms: "The contest," Thucydides has him say, "is a momentous one for you—whether you are to shatter the hope which the Peloponnesians have in their fleet, or to bring closer home to the Athenians their fear about the sea."⁴⁵

If events of the first year of the war did much to alleviate Athenian fears, they did little to encourage Spartan hopes. Sparta's appeals for aid went unfulfilled, and, as the war entered its second year, the Spartans were in no better position to engage the enemy at sea than they were when the war began. Hence, they had no choice but to invade Attica a second time, and this invasion, the most ambitious of the entire Archidamian War,⁴⁶ lasted almost forty days and penetrated as far as the vicinity of Laurium. During this summer the plague made its first appearance at Athens, but, even in the face of these dual adversities, the Athenians carried the war to the enemy. A fleet of one hundred Athenian and fifty allied ships was sent to ravage the Peloponnesus, and attacks were mounted on Epidaurus, Troezen, Hermione, Halieis, and Prasiae.⁴⁷ Before the summer ended, however, the disastrous effects of the plague coupled with the devastation of Attica led to a reaction against Pericles and his policies. Ambassadors were dispatched to Sparta in

⁴³ On the establishment of the reserve, see Thucydides 2.24.1–2; and, on the expenditure of the fund, see Thucydides 8.15.1.

⁴⁴ On Athenian activities with respect to Perdiccas, see Thucydides 2.29.4–7; with respect to Aegina, see Thucydides 2.27.1–2; and Diodorus 12.44.2–3; with respect to Euboea, see Thucydides 2.26.1, 2.32.1; and, with respect to the Peloponnesian coast, see Thucydides 2.17.4, 2.23.2–3, 2.25.1. It is worth noting that the fleet dispatched in the following year also consisted of one hundred Athenian and fifty allied vessels; Thucydides 2.56.1–2. The size of these fleets accords well with Pericles' boast to the Athenians that the enemy would not be able to acquire naval experience, because every time the Peloponnesian fleet sailed it would be confronted by many ships; Thucydides 1.142.7–8.

⁴⁵ Thucydides (Loeb edn.) 2.89.10.

⁴⁶ Thucydides 2.47.2, 2.55.1–2.

⁴⁷ Thucydides 2.56.1–6.

an effort to end the war. As has often been noted, Thucydides is depressingly vague about the details of the ensuing peace negotiations.⁴⁸ All we really know is that nothing came of them, and we must conclude that the Spartans were in no mood to compromise. Surely they drew confidence from the plague and the hardship this inflicted on the enemy, and those Spartans who believed that invasion of Attica would end the war quickly could argue that this strategy was already wearing the Athenians down.

Sparta's unwillingness to compromise on this occasion, however, must be considered in a broader context. At the same time that they were refusing Athenian peace proposals, the Spartans were preparing their first substantial naval expedition of the war. Although none of the aid they anticipated at the outset of the war had materialized, they were able in the summer of 430 to put to sea a fleet of one hundred ships under the command of Cnemus. What exactly they hoped to accomplish with this fleet we do not know. Cnemus made a half-hearted attack on Zacynthus, but sailed away without achieving anything.⁴⁹ Whether the Spartans expected a mere display of force to produce results, whether they were serving notice that they did have ships and were not afraid to employ them, or whether they had no further aim than to gain experience for their crews we cannot say. The essential point is that the dispatch of this fleet was not an isolated event. Shortly after Cnemus returned from Zacynthus an embassy left Sparta for Persia; the Spartans hoped Artaxerxes would contribute money and join the war. The ambassadors had the additional mission of persuading Sitalces to desert his Athenian ally and send a force to relieve Potidaea. Much has been made of this embassy's failure to reach its destination. Seized by Sadocus, the son of Sitalces, the ambassadors were transported to Athens where they were immediately executed without trial.⁵⁰ The importance of this embassy lies not only in its dispatch, but also in its timing, just after the outbreak of the plague at Athens. If the Spartans were soliciting aid from Artaxerxes and Sitalces, they were obviously not convinced that invasion of Attica alone would achieve victory over the enemy—not even an enemy suffering the effects of a debilitating plague.

Sometime during the winter of 430–29 the Athenians sent Phormio with twenty ships to take up station at Naupactus. This was an extraordinary measure; only on rare occasion were ships dispatched during the winter months.⁵¹ Apparently Athens anticipated enemy naval activity in these western waters, but Sparta appears to have had no such intention for the approaching campaigning season. The

⁴⁸ Thucydides 2.59.1–2. Diodorus adds nothing significant; Diodorus 12.45.5. Donald Kagan has discussed the matter fully and, on the basis of Aristophanes *Acharnians* 646–54, has concluded that the return of Aegina to the Aeginetans was one of Sparta's conditions; *Archidamian War*, 81–84.

⁴⁹ Thucydides 2.66.1–2.

⁵⁰ Thucydides 2.67.1–4; and Herodotus 7.137. This was, as Lewis has pointed out, a well-planned mission, including three Spartans, a Corinthian, a Tegean, and an Argive; Lewis, *Sparta and Persia*, 64. Thucydides seems to imply that there had been earlier contact between the Spartans and Persians, for, upon their arrival in Asia Minor, these ambassadors were to be escorted to the king by Pharnaces.

⁵¹ Thucydides 2.69.1. But Diodorus does not mention the winter sailing; Diodorus 12.47.1. Lionel Casson has listed only four instances reported by Thucydides and noted that in each case the circumstances were exceptional; Casson, *Ships and Seamanship in the Ancient World* (Princeton, 1973), 270 n. 3. To this list, add Thucydides 2.33.1; see note 40, above.

Spartans did resolve to reduce Plataea,⁵² but, so far as we know, they had no other plans for offensive operations, either by land or sea. That the Spartans did not even contemplate an invasion of Attica in 429 is especially significant. This third year of the war was crucial for those Spartans who believed that such invasions would end the war in three years or less. A variety of reasons for the absence of an invasion in this year have been offered. It has, for example, been suggested that the Spartans feared contracting the plague, that after two years of devastation they could inflict little additional damage, and that they expected the Athenians, who would not come out from behind their fortification walls to defend their own fields, would now be forced to come out to aid their Plataean allies.⁵³ Although any or all of these considerations may have contributed to Sparta's decision not to invade Attica in this year, that decision all but forces us to conclude that, if they had not realized it earlier, they were now well aware that any hope they may have had of winning the war by invading Attica was illusory.

The only other offensive operation undertaken by Sparta and its allies in this year reinforces this conclusion. As the summer progressed, the allies became increasingly restless and finally prodded the Spartans to act. Just after mid-summer, the Ambraciots proposed a plan they hoped would lead to the subjugation of the whole of Acarnania and its detachment from alliance with Athens. The Corinthians were eager to undertake this operation, but the Spartans, Thucydides says, were reluctant to do so. Only after they were persuaded by Ambraciot arguments that the plan would lead not only to the occupation of Acarnania, but also the takeover of Cephallenia, Zacynthus, and, possibly, Naupactus did the Spartans agree to equip a fleet and send 1,000 hoplites.⁵⁴

The Peloponnesian plan was ambitious and led to the first naval engagement of the war. It called for a joint land and sea offensive under the command of Cnemus, the Spartan nauarch. He was dispatched to Acarnania with the hoplites and a few ships, while the allies were ordered to assemble the fleet at Leucas. As Cnemus moved inland with the army, the fleet was to cruise along the coast, harassing the inhabitants of the area so that they could not aid their countrymen in the interior. The primary objective of the army was Stratus, the largest city in Acarnania, whose conquest, Cnemus believed, would bring the whole territory over to the Peloponnesians. Unfortunately, nothing went as planned. Cnemus was not victorious at Stratus; the cavalry promised by Perdiccas arrived too late to assist in the attack; and the fleet failed to assemble at Leucas, much less harass the coastal areas.⁵⁵

The key to the failure of the mission was the presence of Phormio and his twenty ships at Naupactus. The Peloponnesians had clearly underestimated the impor-

⁵² Thucydides 2.71.1; and Diodorus 12.47.1–3.

⁵³ Kagan has discussed Spartan motives and suggested that the attack was forced on the Spartans by their Theban allies; *Archidamian War*, 102–03.

⁵⁴ Thucydides 2.80.1–7. Noteworthy is the secret agreement of Perdiccas to aid the Spartans; Thucydides 2.80.7.

⁵⁵ For the land campaign, see Thucydides 2.81–82. Diodorus adds little of value; Diodorus 12.47.4–5. Also see R. L. Beaumont, "Corinth, Ambracia, Apollonia," *Journal of Hellenic Studies*, 72 (1952): 62–73; and Nicholas G. L. Hammond, "Military Operations in Amphilochoia," in his *Studies in Greek History*, 471–85. Beaumont has provided valuable observations on the topography of the area and a summary of military activity during the Archidamian War.

tance of this fleet and the resolve of its commander. As Cnemus and his army moved inland, forty-seven Corinthian ships sailed through the Gulf of Corinth to join up with the fleet assembling at Leucas. The Corinthians never expected that Phormio, with his small command, would attempt to hinder their passage. So confident were they, in fact, that they had not even bothered to equip their ships for fighting at sea. But Phormio did attack. His superior tactics and better knowledge of the winds, coupled with Peloponnesian inexperience and unpreparedness, enabled him to capture twelve enemy ships without incurring any losses. As the Corinthian ships that survived straggled on to Cyllene, news of Phormio's victory reached Sparta. As noted above, the Spartans could neither understand the defeat nor believe that inexperience had anything to do with it. They blamed the loss, rather, on *malakia*, and in this they were absolutely sincere. The dispatch of Brasidas, Timocrates, and Lycophron to serve as advisers to Cnemus was intended to remedy this "lack of energy," and preparations to resume the offensive at sea were begun immediately.⁵⁶ This time they were more thorough, but no more effective.

The second naval engagement of 429 must rank as one of the most significant battles of the Archidamian War. While a fleet of seventy-seven Peloponnesian ships assembled at Cyllene, Phormio sent a hurried call to Athens for reinforcements. Twenty additional ships were dispatched but with orders to sail first to Crete. Until they arrived, Phormio had to make do with the twenty ships under his command. When preparations were completed, the Peloponnesian fleet anchored off Achaean Rhium, while the Athenians took up station off Molycrian Rhium. The Peloponnesians wished to provoke an engagement before Athenian reinforcements could come up, and their plan was skillful. When Phormio refused to initiate hostilities, they sailed toward Naupactus, forcing him to put to sea to defend his base. Nine Athenian ships were quickly separated from the remaining eleven and either captured or disabled, but this success made the inexperienced Peloponnesian crews over-confident. Phormio regained the initiative, recovering the lost ships and capturing six enemy triremes. After the dead were exchanged, each side set up a trophy commemorating its victory, but this would fool no one. The Athenians had overcome odds of nearly four to one, and they remained masters of the sea beyond all doubt. Even though they still greatly outnumbered the Athenians, whose reinforcements had not yet arrived, the Peloponnesians refused to offer battle. Rather, they awaited the cover of darkness before the Leucadians sailed home and the rest of the fleet proceeded to Corinth.⁵⁷

We can only imagine how Sparta reacted to Phormio's second victory, for Thucydides does not tell us. Clearly, however, neither Cnemus nor his advisers were anxious to return home on this sour note; accordingly they agreed to a proposal of the Megarians, to launch the forty ships at Nisaea and attack the

⁵⁶ For these events, see Thucydides 2.83–85.

⁵⁷ Thucydides 2.86–92; and Diodorus 12.48.1–3. The battle has been much discussed, and Gomme's commentary on these passages of Thucydides remains invaluable; *A Historical Commentary on Thucydides*, 2: 222–37. Also see August Köster, *Studien zur Geschichte des antiken Seewesens* (Leipzig, 1934), 81–96; and William L. Rodgers, *Greek and Roman Naval Warfare* (Annapolis, 1937), 129–36. Kagan has analyzed the significance of Phormio's victory; *Archidamian War*, 115.

Piraeus by sea. Men bearing naval equipment marched across the Isthmus, but, by the time the ships put to sea, the objective had been scaled down to an attack on Salamis. The island was ravaged, three enemy triremes were captured, and the Athenians were thrown into a panic.⁵⁸ The operation certainly did not change the course of the war, but Cnemus, Brasidas, and Lycophron were now able to return home with something besides defeat to show for their efforts.

This attack on Salamis ended the third campaigning season of the war, but, despite Thucydides' pronouncement that no one believed that the Athenians could hold out for more than three years, the war was still far from over. The Athenians had paid dearly in men and money, but they had withstood invasion of their territory, the ravages of the plague, and the loss of Pericles. They had demonstrated, moreover, their overwhelming superiority in naval warfare. On the Peloponnesian side, most of the damage had fallen on Sparta's allies; the Spartans had not suffered severely, but they had little to show for their efforts. If in the past they had deluded themselves into believing they could compete with the Athenians on the sea, Phormio had proven otherwise, and the size of the Peloponnesian fleet had declined rather than grown as they had earlier anticipated. In short, three years of warfare had shown that Athens could not easily, if at all, be reduced by invasion, that neither the Persians nor friendly Greek states were eager to come to Sparta's aid, and that Athenian naval experience was an even more formidable obstacle than the Spartans imagined.

THESE LESSONS WERE NOT LOST ON THE SPARTANS, and before the next campaigning season began they adopted a policy to widen the war, particularly its naval side. In the spring of 428 the Spartans once again invaded Attica, but even as they did so they were encouraging revolt at Mytilene on the island of Lesbos. We do not know when they decided to become involved in affairs there, but the decision to do so must have been made not long after the attack on Salamis late in 429. Since the revolt entailed time-consuming preparations, such as sending abroad for grain and building ships and walls, the revolt must have been contemplated for some time before it actually broke out in June 428. The Lesbians would scarcely have gone ahead with these preparations without some assurance from Sparta.⁵⁹

Encouraging revolt at Mytilene was a bold and daring measure. In view of the humiliation at sea in the previous summer, it was not something to be undertaken lightly. Lesbos was, after all, an ally of Athens, an island located far away from the Peloponnesus and in waters long regarded as an Athenian lake. The issue of supporting revolt there seems to have been a divisive one at Sparta, and interest in the project vacillated from lukewarm to enthusiastic throughout the summer of

⁵⁸ Thucydides 2.93–94; and Diodorus 12.49.1–4.

⁵⁹ On the events themselves, see Thucydides 3.1–2. On the chronology, see Gomme, *A Historical Commentary on Thucydides*, 2: 237, 252. According to Gomme's chronology, the attack on Salamis occurred about November 429, and Spartan involvement at Mytilene was certainly known there before the revolt broke out the following June. On the unlikelihood of the Lesbians revolting without promises of assistance from Sparta, see Thucydides 3.2.1; Thucydides tells us they wanted to rebel before the war but did not because the Spartans refused to ally with them. For the dispatch of an adviser to Mytilene before the outbreak of the revolt, see Thucydides 3.5.4.

428. In June, a Spartan ambassador brought encouraging news to the rebels, but, when Lesbian ambassadors arrived at Sparta in July, no immediate pledge of assistance was forthcoming. They were ordered to present their case at Olympia when members of the Peloponnesian League would be assembled there for the celebration of the games in August. When the Peloponnesian allies voted to assist the Lesbians, the Spartans eagerly embraced the vote.⁶⁰ This was an opportunity they could not afford to pass up, because if all went well, an important ally would be detached from the Athenian empire and the first addition to the Peloponnesian fleet since the war began would be made.

For the second time in two years the Spartans prepared for a combined land and sea offensive against the enemy. This time the objective was not some remote corner of the Greek world, but Athens itself. The allies present at Olympia were directed to prepare two-thirds of their forces and assemble at the Isthmus as quickly as possible. The Spartans arrived first and began constructing machines to haul ships across the Isthmus, but their allies gathered slowly. Spartan indecisiveness in firmly committing aid to the Lesbians earlier in the summer had cost precious time; by now the campaigning season was drawing to a close, and crops had to be harvested. As a result, many allies did not join the expedition, and those who did were treated to an unexpected show of strength by the Athenians. Acting on advice from the Lesbians, the Spartans had assumed that Athens, weakened by the plague and with one fleet of thirty ships on station at Lesbos and another of the same size cruising the Peloponnesus, would offer little opposition unless it recalled the fleets. Without resorting to this expedient, however, the Athenians hastily put one hundred additional ships to sea. Confronted with this display of force, the mission was dismissed as impractical and hastily called off. No longer threatened by invasion the Athenians were able to deal directly with the Lesbian rebels. A force of 1,000 hoplites serving as rowers arrived at the scene, and by the onset of winter Mytilene was encircled, cut off by land and sea.⁶¹

If Spartan ardor was at all cooled by these setbacks, there is no evidence in Thucydides; on the contrary, the Spartans were still determined to capitalize on events at Mytilene. It was imperative that the rebels not capitulate before the next campaigning season began, and they were encouraged to hold out. During the winter, probably toward the end of February, Salaethus was sent to take charge of things at Lesbos. Evading the Athenians, he brought news that, come spring, Attica would be invaded and a fleet would be sent to relieve the blockade.⁶² For the third time in three years the Spartans had plans for simultaneous land and sea operations against the enemy; indeed, the year 427 marks the high point of Peloponnesian naval activity in the Archidamian War.

The invasion of Attica and the naval expedition to Mytilene, though separated by

⁶⁰ On the events themselves, see Thucydides 3.8–15. On the chronology, see Gomme, *A Historical Commentary on Thucydides*, 2: 259. Kagan has argued that the Spartans were eventually won over by the speech of the Lesbians, but, even if such a speech had been delivered, it hardly contains anything the Spartans did not already know. See Kagan, *Archidamian War*, 139–41; and Thucydides 3.9–14.

⁶¹ For Sparta's military operations, see Thucydides 3.15–16; and Gomme, *A Historical Commentary on Thucydides*, 2: 270–78. On the encirclement of Mytilene, see Thucydides 3.18.3–5.

⁶² Thucydides 3.25.1–2.

many miles, was conceived as a joint operation. The fleet, under the command of Alcidas, was dispatched first, and the invasion followed. It was hoped that the land attack would deter the Athenians from sending a fleet to intercept the force heading for Lesbos. Initially all went as planned. While the army ravaged Attica at will, the fleet managed to avoid detection by the Athenians. Yet the crossing was not as expeditious as it might have been. Precious time was wasted, and, when he arrived at Myconos, Alcidas learned that Mytilene had fallen. This turn of events jeopardized the entire operation. The original plan had called for the army to remain in Attica until the fleet reached its destination, but, when no news came from Mytilene and its food supply was exhausted, the army was forced to withdraw.⁶³ For the third time in three years a land and sea campaign had ended in failure.

Much of the blame for this failure must be placed squarely on the shoulders of Alcidas. Thucydides pictures him as a thoroughly unimaginative Spartan who allowed one opportunity after another to slip through his fingers. He had wasted valuable time crossing over from the Peloponnesus, and, on learning that Mytilene had already succumbed, he declined to attack the city and take the Athenians by surprise. Moreover, he also refused to seize a city on the mainland of Asia Minor for use as a base for promoting other revolts from Athens, and the cruelty he displayed in putting to death potentially friendly Teans did nothing to aid Sparta's cause among the eastern Greeks.⁶⁴ In fairness to the man, however, we do not know precisely what his orders were, and his mission was not easy under any circumstances. Presumably, he was to lift the siege of Mytilene, as promised; and, had he arrived in time, he might, with assistance from the Lesbian fleet, have had some chance of success. Without such assistance he faced a difficult task; the fleet he commanded was no larger than the Athenian force maintaining the blockade,⁶⁵ and in view of Peloponnesian naval setbacks of the previous summer this could hardly inspire confidence. We do not know what, if any, contingency orders he may have had, but his decision to return to the Peloponnesus without once engaging the enemy did not, so far as we know, give rise to displeasure at Sparta.⁶⁶

⁶³ On the invasion of Attica, see Thucydides 3.26.1–4. On the fleet crossing the Aegean, see Thucydides 3.29.1–2.

⁶⁴ For a description of Alcidas' behavior in the east, see Thucydides 3.29–33. For a discussion of his character, see H. D. Westlake, *Individuals in Thucydides* (Cambridge, 1968), 142–47.

⁶⁵ The Athenians originally dispatched a fleet of forty ships under the command of Cleippides and two others, and this force was later reinforced when Paches was dispatched with 1,000 hoplites serving as rowers, presumably on five triremes; Thucydides 3.3.2, 3.18.3. For what it may be worth, Diodorus says the fleet commanded by Cleippides, whom he mistakenly calls Cleinippides, collected reinforcements from the allies before it arrived in Lesbos; and he gives the size of Alcidas' fleet as forty-five ships; Diodorus 12.55.3, 12.55.6. Thucydides gives the number of ships in Alcidas' fleet as forty in three places, although he does say forty-two at one point; Thucydides 3.16.3, 3.29.1, 3.69.1, but 3.26.1. For a discussion on the size of Alcidas' fleet, see Gomme, *A Historical Commentary on Thucydides*, 2: 288.

⁶⁶ Westlake has suggested that the appointment of Brasidas to serve as adviser to Alcidas on his return from Ionia is a reflection of Spartan disapproval of his performance; Thucydides 3.69.1; and Westlake, *Individuals in Thucydides*, 145. This is certainly possible; but Alcidas had been in the east for some time, and the decision to send the fleet to Corcyra had apparently been made in his absence; Thucydides 3.69.2. He would have had to have been brought up to date, and the appointment of Brasidas may have been made for this reason. If Alcidas had fallen into disfavor at Sparta, it is unlikely that he would have been one of three *oikistai* sent out to the colony of Heracleia Trachinia, which was founded in the following year; Thucydides 3.92.5.

Sparta's inability to exploit the revolt at Mytilene must be regarded as one of its most serious failures in the entire Archidamian War. The appearance of a Peloponnesian fleet in Ionian waters was totally unexpected. It generated fear and anger at Athens and generated momentary hope among Greek states disenchanted with Athenian domination.⁶⁷ Yet there were no long-range gains, only long-range losses. Alcidas may have picked up some contributions to the Peloponnesian war effort,⁶⁸ but otherwise Sparta and its allies had little to show for their activities. The revolt was crushed, the Lesbian navy taken over by the Athenians, and cleruchs sent to the island.⁶⁹ Athens' ability to suppress the revolt while withstanding an invasion of its territory, coupled with Sparta's inability to provide any meaningful assistance to the rebels, provided a lesson that could hardly be lost on the Greeks in the east. Even though anti-Athenian feeling seems to have been high among them,⁷⁰ the possibility of other revolts had been lessened. Indeed, not for another fifteen years did Peloponnesian ships once again ply Ionian waters.

Assistance from the eastern Greeks was now unlikely, but the Spartans quickly seized another opportunity to augment their naval forces. No sooner did the Spartans learn that Mytilene had capitulated when they began, this time with no hesitation, laying plans for further use of Alcidas' fleet. The prize they sought was Corcyra with its large naval reserves. The series of events leading up to Sparta's involvement at Corcyra had begun long before. In 433 the Corinthians had taken some 1,000 prisoners in their victory over the Corcyreans at Sybota. Most were slaves and immediately sold, but 250 of these prisoners, among them the most influential men in the city, were removed to Corinth, where they were treated with consideration. The Corinthians hoped that, when repatriated, they would work to bring Corcyra over to the Peloponnesian cause. We do not know how long they were in Corinthian custody, but, when they returned home, they began working against the Athenians.⁷¹ By the summer of 427 there was growing sentiment at Corcyra in favor of closer ties with the Peloponnesians. There was also opposition, of course, and in the end an impossible compromise was struck. The Corcyrean assembly voted to continue the defensive alliance with Athens but, at the same time, to renew their former friendship with the Peloponnesians. A bitter and hotly contested civil war erupted, the excesses of which led Thucydides to one of his most famous digressions, and the Spartans hastened to take advantage of the situation.⁷²

On the return voyage from Ionia, Alcidas apparently did not even stop in Laconia, but sailed straight for Cyllene. On arriving, he found thirteen Leucadian

⁶⁷ On anti-Athenian feeling in the east, see Thucydides 3.31.1.

⁶⁸ Thucydides 3.50.1–3. The evidence for contributions to the war effort comes from a Spartan inscription; Russell Meiggs and David M. Lewis, *A Selection of Greek Historical Inscriptions* (Oxford, 1969), Nr. 67. But the dating is not absolutely certain. Frank E. Adcock has argued for the year 427, but other dates, including 396–95 B.C., have been suggested; Adcock, "Alcidas ἀργυρολόγος," in *Mélanges Gustave Glotz*, 1 (Paris, 1932): 1–6; and Meiggs and Lewis, *Greek Historical Inscriptions*, 183–84.

⁶⁹ Thucydides 3.50.1–3.

⁷⁰ Thucydides 3.31.1; and Meiggs and Lewis, *Greek Historical Inscriptions*, Nr. 67. Line 9 of the inscription specifically mentions Chians friendly to Sparta. There was also trouble at Notium; Thucydides 3.34.1–4.

⁷¹ Thucydides 1.55.1, 3.70.1; Diodorus 12.57.1–2; and Gomme, *A Historical Commentary on Thucydides*, 2: 359–60.

⁷² Thucydides 3.70.3–6.

and Ambraciot ships ready to join his fleet and Brasidas waiting to assume the role of adviser for a new mission.⁷³ The Spartan plan called for the fleet of fifty-three ships to move on to Corcyra before reinforcements could be summoned from Athens to assist the small fleet of twelve ships stationed at Naupactus. The Peloponnesians reached Corcyra before Athenian reinforcements arrived, but found the twelve ships from Naupactus, commanded by Nicostratus, already anchored in the harbor. Although there was much confusion in the city, the presence of the Athenians had shifted the initiative from the supporters of Sparta to the supporters of Athens.⁷⁴ As a result, Nicostratus could count on some assistance from the Corcyrean fleet, and Alcidas was now faced with the prospect of engaging a sizable enemy naval force.

As the Peloponnesian fleet approached, the Corcyreans hastily manned sixty ships, but so volatile were the issues separating the two factions that the fleet sailed out in total disarray. Two ships immediately surrendered to the Peloponnesians, while on others crewmen expended as much energy fighting among themselves as against the enemy. Aware of this, Alcidas prepared to send twenty ships against the Corcyreans, while reserving thirty-three triremes for battle against the Athenians. The Corcyreans offered little resistance; thirteen of their ships were captured. But, when the Athenians sank one Peloponnesian ship, Alcidas reassembled his fleet, and the Corcyrean and Athenian ships retreated into the harbor. The battle was over and the city itself ripe for attack. Once again, however, Alcidas proved to be an irresolute commander. The Peloponnesians retired to Sybota. On the following day, they ravaged the territory around Leucimne, but again refused to attack the city directly or engage thirty Corcyrean ships that put to sea. By noon they had decided to withdraw, and, after receiving a signal toward nightfall that sixty Athenian ships were approaching, their single-minded goal became a safe return. By sailing close to shore at night and hauling their ships across the isthmus at Leucas, they managed to avoid the Athenians and make it back to the Peloponnesus.⁷⁵

The year 427, it bears repeating, marks the high point of Peloponnesian naval activity in the Archidamian War. Although their fleet had sailed from Asia Minor to Corcyra and could boast a minor victory over the disorganized Corcyreans, Sparta and its allies had not shown they could compete with the Athenians at sea. Indeed, events of this year had demonstrated the very opposite. They had steadfastly shied away from any confrontation with the Athenian fleet, and, although it is easy to blame Alcidas, his reluctance to engage the Athenians may reflect his wisdom as much as his incompetence. On the one occasion when he did so, it must be remembered, one of his thirty-three ships was sunk by a force of only twelve Athenian ships. Evidently, Athenian experience was still a decisive factor. Moreover, the number of ships Sparta and its allies were able to put to sea had declined by nearly one-half, fifty-three in 427 as opposed to one hundred in 430. Yet, for

⁷³ Thucydides 3.69.1–2. The decision to attempt to capitalize on events at Corcyra seems to have been made while Alcidas was in the east with the fleet; see note 65, above.

⁷⁴ Thucydides 3.69.2, 3.75.1–5.

⁷⁵ On the battle and its aftermath, see Thucydides 3.76–81.

our purposes, the important consideration is that they twice put to sea in this year, and in each instance with the expectation that success would significantly add to the Peloponnesian fleet. By contrast, their only activity on land was an invasion of Attica for the primary purpose of distracting the Athenians while the fleet crossed the Aegean. Clearly, these are not the actions of a state convinced it could win the war by invading Attica.⁷⁶

It is likely that Archidamus passed away sometime during the winter of 427–26. On his death Pleistoanax was recalled, but it does not follow, as has sometimes been argued, that Sparta was now inclined to come to terms with Athens. As Donald Kagan has noted, there is no evidence of any peace initiatives,⁷⁷ and, as the new campaigning season opened, the Spartans certainly had plans, though not very decisive ones, for continuing the war. Even though the plague had once again broken out at Athens during the winter, at the beginning of spring, Peloponnesian forces assembled at the Isthmus for an invasion of Attica. As they were assembling, earthquakes occurred, and they decided to turn back. It is difficult to believe that the Spartans really expected another invasion of Attica to resolve the war,⁷⁸ but we know of no other plans for offensive operations. In contrast to the previous year, the Peloponnesian navy was inactive; not a single Peloponnesian ship was dispatched anywhere in 426. It was only the persuasion of their friends and allies—the Trachinians and Dorians in one instance and the Aetolians and later the Ambra-ciots in the other—that finally moved the Spartans to action. It is more than mere coincidence that in each case their decision was heavily influenced by naval concerns and considerations.

Early in the summer of 426, the Spartans, acting on the appeal from the Trachinians and the Dorians of Doris, proceeded to establish a colony at Heracleia Trachinia.⁷⁹ They wished to aid both states in their struggle against the Oetaeans, but other considerations were involved. The Spartans were fully cognizant of the

⁷⁶ It may be worth noting that, toward the end of this summer of Peloponnesian naval activity, the Athenians first dispatched a fleet to Sicily to prevent the importation of grain into the Peloponnesus and to test whether they could bring the affairs of Sicily under their control; Thucydides 3.86.4. Timothy E. Wick has argued that the main aim of the mission was to end the flow of grain to Megara through Pegae, but, as Westlake has pointed out, the Athenians seemed to have differed over the precise aims of the mission, and they could not completely discount the possibility of Sparta receiving aid from the west; Wick, "Megara, Athens, and the West in the Archidamian War," *Historia*, 28 (1979): 1–14; and Westlake, "Athenian Aims in Sicily, 427–424 B.C.," 104, 110–13. Besides, as Georg Busolt long ago recognized, Syracuse was in a better position to aid the Spartans in 427 than it had been at the outbreak of the war; Busolt, *Griechische Geschichte*, 3 (Gotha, 1903): 1055.

⁷⁷ On the recall of Pleistoanax, see Kagan, *Archidamian War*, 193–95. Kagan here has argued forcefully against the use of Aristophanes *Acharnians* 646–54 as proof of Spartan peace initiatives in this year. Nowhere does Thucydides note or comment upon the death of Archidamus, but he did not lead the invasion of Attica in the spring of 427. Spartan forces on that occasion were commanded by Cleombrutus, and the aborted invasion of 426 was led by Agis; Thucydides 3.26.2, 3.89.1.

⁷⁸ Thucydides 3.89.1; and Diodorus 12.59.1–2. Gomme has suggested that one reason plans for the invasion were abandoned is that the Spartans lacked enthusiasm for it; Gomme, *A Historical Commentary on Thucydides*, 2: 395. But Thucydides leaves no doubt that the earthquake was severe, and it may well have led the Spartans to change their plans.

⁷⁹ On the foundation of Heracleia, see Thucydides 3.92.1–6; and Diodorus 12.59.3–5. The number of inhabitants—10,000—given by Diodorus is unrealistic, but Thucydides implies that it was sizeable. Gomme has regarded the founding of the colony as proof that the Spartans were beginning to understand that their original strategy of invading Attica would not win the war and that other means would have to be found, a position that Kagan has adopted as well; Gomme, *A Historical Commentary on Thucydides*, 2: 394–99; and Kagan, *Archidamian War*, 195–97.

advantages of the site, particularly with respect to naval operations. They recognized it as well suited for equipping a fleet and attacking Euboea and for conducting expeditions along the coast toward Thrace. Sparta's seriousness in the venture is evident from the size of the colony and the inclusion of Spartan citizens, as well as *periokoi*, among the original settlers. Thucydides assures us that the Spartans clearly anticipated that the colony would be used as a base for naval operations against the enemy. In this connection it may be significant that Alcidas, who had practical naval experience, was one of the three *oikistai* sent out with the colony and that shortly after the colony was founded dockyards were constructed there. Although Heracleia proved useful as a stopping place for Peloponnesian land forces on their way to Thrace later in the war,⁸⁰ it seems never to have been used as a base for naval operations. This has led some scholars to question Thucydides' report of Sparta's original intent,⁸¹ but, if the founding of Heracleia is considered in connection with the only offensive operation undertaken against the Athenians in this year, his report gains credibility.

In the spring of 426 Demosthenes had set out from Athens with a fleet of thirty ships, cruised the Peloponnesus, and, after ravaging the territory around Leucas, sailed on to Naupactus. There he was persuaded by the Messenians to attack Aetolia. The campaign accomplished nothing, but in retaliation the Aetolians appealed to the Corinthians and Spartans to launch an attack on Naupactus.⁸² Toward autumn a force of 3,000 allied hoplites, including 600 from the newly established colony at Heracleia, marched into the area. They achieved some success on the outskirts of Naupactus, but, thanks to the resourcefulness of Demosthenes, they were not able to take the city. Apparently, the Spartan commander, Eurylochus, did not wish to return home without success of some sort, and he was persuaded by the Ambraciots to attack Amphilocian Argos in an effort to secure control of all of Amphilocia and Acarnania as well. The campaign continued into the winter, when the Peloponnesian force suffered a staggering defeat and Eurylochus was killed.⁸³

The connection between this campaign and Spartan efforts to conduct war at sea is more intimate than initially meets the eye. In a very real sense Naupactus was the Spartan Aegina—the eyesore of the Gulf of Corinth.⁸⁴ Its importance as an Athenian naval base was clearly demonstrated initially in 429, when Phormio, with a mere handful of ships, was able to prevent the Peloponnesian fleet from assembling at Leucas according to plan; its importance was reaffirmed in 427, when Spartan efforts to capitalize on events at Corcyra were thwarted by an Athenian

⁸⁰ Thucydides 4.78.1, 5.12.1.

⁸¹ Antony Andrewes and Donald Kagan have both seen the establishment of the colony as an indication of Sparta's plans to expand the land war; Andrewes, "Spartan Imperialism?" in P. D. A. Garnsey and C. R. Whittaker, eds., *Imperialism in the Ancient World* (Cambridge, 1978), 95–99; and Kagan, *Archidamian War*, 196. Interestingly, Livy reports that in 191 B.C. the area around Heracleia was thickly wooded with tall trees; Livy 36.22.

⁸² On Demosthenes' campaign in Aetolia, see Thucydides 3.94–98; and Diodorus 12.60.1; and, on the Aetolian appeals to Corinth and Sparta, see Thucydides 3.100.1.

⁸³ For a description of the Peloponnesian campaign, see Thucydides 3.100.2–3.102.7, 3.105–14. Also see Diodorus 12.60.2–6; and Hammond, "Military Operations in Amphilochia," 476–84.

⁸⁴ It is worth noting in this connection that in 401 B.C. the Spartans drove the Messenians from Naupactus and settled friendly Locrians there; Diodorus 14.34.2–3.

fleet based at Naupactus. Hence, it is not surprising that in 426 the Spartans attempted to take the city. The operation takes on added significance, however, in view of the civil war at Corcyra, which was still far from resolved. The Spartans had not yet abandoned hopes of exploiting the Corcyrean struggle to their own advantage.

After the withdrawal of the Athenian fleet from Corcyra in 427, Sparta's supporters had managed to escape from the city and move to the mainland opposite the island, where they seized territory belonging to Corcyra. They began plundering their homeland so effectively that famine soon resulted. The exiles appealed to both Corinth and Sparta for assistance, but, when no help came, they hired mercenaries and procured vessels, returned to Corcyra, and captured and fortified Mt. Istone. From their fortifications they dominated the countryside and conducted raids against the city.⁸⁵ All of this was known to the Spartans when they attacked Naupactus in the autumn of 426. Thucydides does not say so, but it is difficult to believe that this attack was not part of a larger scheme designed to bring Corcyra once and for all under Peloponnesian control. We know that the Spartans had such plans ready to be set in motion a few months later when the next campaigning season began. Although they had failed to take Naupactus, and the Athenians had a fleet stationed there, sixty Peloponnesian ships were ready to sail to Corcyra early in the spring of 425.⁸⁶ The two operations dovetail together too neatly to be anything but individual parts of a single plan.

The invasion of Attica in the spring of 425 must be seen as part of the same plan. The Spartans could hardly have expected this invasion to alter the course of the war, when four previous invasions had failed to do so. Further damage could, of course, be inflicted upon the enemy, but the Athenians had long since demonstrated their ability to endure such destruction. Nor is it insignificant that this invasion was launched earlier in the season than any other during the war.⁸⁷ Time was of the essence, for during the winter of 426–25 the Athenians had voted to dispatch a fleet to Sicily. A few ships were sent out immediately; the remainder were to be sent in the spring of 425. The Spartans were undoubtedly anxious to secure control of Corcyra before the rest of the fleet should be sent out and while only twenty ships were on station at Naupactus.⁸⁸ In short, this invasion is most easily explicable as a diversionary strike, much like the invasion of 427, when the Spartans hoped to divert Athenian attention from the fleet sailing to Lesbos. Now, if all went well, the land attack would occupy the Athenians sufficiently to allow the Peloponnesian fleet

⁸⁵ Thucydides 3.85.2–4.

⁸⁶ Thucydides 4.2.3, 4.3.1, 4.8.2.

⁸⁷ This is the only invasion of the war that took place in the spring "before the grain was ripe" (*ὑπὸ δὲ τοὺς αὐτοὺς χρόνους τοῦ ἡρος, πρὶν τὸν σῖτον ἐν ἀκμῇ εἶναι*); Thucydides 4.2.1, 4.6.1. All other invasions came "in summer" (*τὸ θέρος*); Thucydides 2.19.2, 2.47.2, 3.1.1, 3.26.1, 3.89.1. The invasion of 431 came "when the grain was ripe" (*καὶ τοῦ σίτου ἀκμάζοντος*), and that of 428 "when the grain was ripening" (*ἔμα τῷ σίτῳ ἀκμάζοντι*); Thucydides 2.19.2, 3.1.1.

⁸⁸ For the Athenian decision to send forty additional ships to Sicily, with a few to be dispatched immediately and the remainder to follow later, see Thucydides 3.115.3–5. The rest did leave in the spring; Thucydides 4.2.2. On the twenty vessels stationed at Naupactus, see Thucydides 3.114.2.

to gain control of Corcyra, where, Thucydides assures us, they expected quick results.⁸⁹

The fleet under the command of Thrasymlidas reached its destination, but we hear nothing of its success or failure; in any case, it did not remain there long enough to accomplish much. While it was at Corcyra and even as the Peloponnesian army ravaged Attica, the Athenians dispatched the rest of the fleet destined for Sicily—forty ships under the command of Eurymedon and Sophocles—with orders to put in first at Corcyra. Demosthenes accompanied the fleet in a private capacity and had been given authorization to use the fleet as he saw fit during the Peloponnesian portion of its journey. Over the objections of Eurymedon and Sophocles, he decided to occupy Pylos, an event that was to alter dramatically the course of the war. To meet this threat, the Spartan army hastily withdrew from Attica, and the fleet was recalled from Corcyra, as the Spartans were determined to oust the Athenians from their territory.⁹⁰

The details of the battle that followed are well known and need not be treated here.⁹¹ The Peloponnesian fleet was defeated, and Spartan troops were trapped on the island of Sphacteria, which the Athenians then blockaded. To relieve this desperate situation the Spartans asked for a truce. Part of the price demanded by the Athenians was that all ships that had taken part in the battle and all other warships in Laconia be surrendered for the duration of the truce. Sixty vessels were turned over to the Athenians, who were to retain them until Sparta's ambassadors returned from Athens.⁹² Although the Spartans offered to desert their allies and grant favorable terms, they found the Athenians unwilling to compromise, and no settlement could be reached. On the expiration of the truce, the Athenians accused the Spartans of violating its terms, and refused to return any of the sixty Peloponnesian ships in their keeping.⁹³ Without these vessels, the Spartans could not rescue the men trapped on Sphacteria, and, when they were not starved into submission quickly enough to suit the Athenians, Cleon was dispatched with a force to take the island by storm. The 292 survivors, including 120 Spartiates, surrendered and were taken to Athens as prisoners.⁹⁴

It has long been recognized that events at Pylos forced the Spartans to change their strategy. So long as the Athenians held these Spartans prisoner and

⁸⁹ Thucydides 4.2.3. George Grote long ago observed that this invasion did not carry the same hope for the Spartans as their earlier efforts had, but he did not go beyond that observation; Grote, *History of Greece*, 6 (New York, 1899): 313. Busolt, however, correctly recognized that this invasion, like that of 427, was a diversionary strike; *Griechische Geschichte*, 1086.

⁹⁰ Thucydides 4.2.2–4.6.1.

⁹¹ For a description of the battle, see Thucydides 4.13–14. The battle at Pylos has been much discussed, most recently by John B. Wilson in his *Pylos 425 B.C.: A Historical and Topographical Study of Thucydides' Account of the Campaign* (Warminster, 1979). I have profited much from H. D. Westlake's study of the battle, which points out the shortcomings in Thucydides' account; Westlake, "The Naval Battle at Pylos and Its Consequences," *Classical Quarterly*, 24 (1974): 211–26.

⁹² Thucydides 4.16.1–3. But, as Westlake has remarked, the Spartan position was not nearly so desperate as a casual reading of Thucydides might suggest; "The Naval Battle at Pylos and Its Consequences," 217–24.

⁹³ Thucydides 4.17.1–4.23.1.

⁹⁴ Thucydides 4.28–39.

threatened them with death should Attica again be invaded, this tactic would have to be abandoned.⁹⁵ Yet the other, and equally important, consequence of the events at Pylos has received scant attention. When the Athenians refused to return the sixty Peloponnesian warships, Sparta and its allies were left without naval forces, and they could no longer conduct operations, however futilely, on the sea. In a single stroke, the Spartans were deprived of both tactics they had thus far employed in the war.

To trace the course of events in the closing years of the Archidamian War is unnecessary here. After Pylos, Spartan opinion was generally inclined toward peace;⁹⁶ only Athenian intransigence perpetuated the war. The Spartan war effort was half-hearted at best. The dispatch of Brasidas to continue the land war in Thrace, it is well to remember, seems to have been motivated as much by a desire to remove helots from the city as to inflict damage upon the enemy. Apart from Brasidas himself, moreover, not a single Spartan citizen took part in the venture, and even his success in capturing Amphipolis did not revive Spartan enthusiasm for all-out war.⁹⁷ Other, similarly desultory measures suggest that some Spartans were still looking for ways to continue the war at sea. Efforts to reach some accord with the Persians were stepped up, but, plagued by indecisiveness, once again came to naught.⁹⁸ It may also be significant that in the winter of 425–24 the Athenians suspected the Chians of planning revolt and that one of Brasidas' first acts after the capture of Amphipolis was to begin the construction of triremes there.⁹⁹ Such activities suggest, but do not prove, that the Spartans were intent upon continuing naval warfare against the Athenians. The capture of the Spartan troops and the loss of the Peloponnesian fleet at Pylos provides, nevertheless, a more logical *terminus* than the Peace of Nicias.

THE DISASTER AT PYLOS OCCURRED in the seventh campaigning season of the war. In these seven years, the Spartans invaded Attica five times; two other invasions were planned but did not take place. Numbers alone do not, however, tell the whole story; we must also take into account the aim of each invasion. Of the two that were planned and then aborted, one had nothing to do with a strategy of winning the war by invading Attica. It was designed solely to divert Athenian attention from events at Lesbos. Similarly, of the five invasions that actually took place, that of 427

⁹⁵ Thucydides 4.41.1. On the importance of the "hostages" for Spartan strategy, see, among others, Kagan, *Archidamian War*, 148; and Cawkwell, "Thucydides' Judgment of Periclean Strategy," 57.

⁹⁶ Thucydides graphically captures Spartan despondency; Thucydides 4.55.1–4. Also see Westlake, "The Naval Battle at Pylos and Its Consequences," 222–24.

⁹⁷ Thucydides 4.80.1–2, 4.108.7.

⁹⁸ Thucydides 4.50.1–2. Gomme has suggested that Spartan indecision resulted from a reluctance to sign away the independence of the Asiatic Greeks in return for Persian aid, but Kagan has claimed that it may have arisen from factional politics at Sparta; Gomme, *A Historical Commentary on Thucydides*, 3: 499; and Kagan, *Archidamian War*, 257–58.

⁹⁹ On Amphipolis, see Thucydides 4.108.6; and, on Chios, see Thucydides 4.51.1. Exiles from Mytilene stepped up their efforts to harass the Athenians in Lesbos and the cities of Aeolis in the summer of 424, but Thucydides makes no connection between their activities and encouragement from Sparta; Thucydides 4.52.1–4. He does mention that Samian exiles were supplying pilots for Peloponnesian ships, but the time reference is somewhat vague; Thucydides 4.75.1.

was intended to distract the Athenians while the Peloponnesian fleet sailed to Mytilene, and that of 425, which lasted but fifteen days,¹⁰⁰ was probably undertaken to keep them occupied while the fleet was enroute to Corcyra. Of five invasions of Attica, only three—those in 431, 430, and 428—were primarily intended to force the Athenians to sue for peace. By way of contrast, in this same seven year period the Spartans employed their land forces in a lengthy attack on Plataea and two land campaigns in Acarnania, where they hoped to conquer Naupactus and thereby curtail Athenian naval activity in the Gulf of Corinth. More to the point, the Spartans sent out a fleet no fewer than six times in these seven years: to Zacynthus in 430; twice engaging the Athenians in the Gulf of Corinth in 429; to Lesbos and then to Corcyra in 427; and to Corcyra in 425. In addition, during the winter of 431–30, a Corinthian fleet sailed to Acarnania. Admittedly, these naval expeditions were notoriously unsuccessful on the whole and resulted in only three battles with the Athenians. Yet, Peloponnesian operations on land were hardly distinguished and seem to have been conducted with no greater vigor than the war at sea. The Spartans ravaged Attica when no opposition was offered, but quickly withdrew from Salamis when the Athenians appeared; and both campaigns in Acarnania ended in failure. Arnold W. Gomme has succinctly summarized Sparta's efforts to this point in the war as "a series of miserable failures and but one success, the inglorious victory over Plataia!"¹⁰¹

If the arguments presented here are valid, we must conclude that in seven years of warfare the Spartans spent less than 120 days ravaging Attica¹⁰² with the single aim of forcing the Athenians into submission. To put it another way, they spent less than 120 days in seven years pursuing the strategy Thucydides tells us they expected would win the war. In this same period, however, they expended at least as much effort putting ships to sea and conducting land operations outside Attica. Indeed, they may well have spent more time reducing Plataea than in invading Attica.¹⁰³

These figures offer little or no support for Thucydides' proposition that they

¹⁰⁰ Thucydides 4.6.2.

¹⁰¹ Gomme, *A Historical Commentary on Thucydides*, 3: 459. R. T. Ridley has pointed out that Athenian armies generally did fairly well in the field, even against the Spartans; Ridley, "The Hoplite as Citizen: Athenian Military Institutions in Their Social Context," *L'Antiquité classique*, 48 (1979): 508–48.

¹⁰² The figure of 120 days is an absolute maximum. Thucydides tells us that the invasion of 430 was the longest of the war, lasting for "almost 40 days"; Thucydides 2.57.2. He gives no precise figures for the invasions of 431 or 428, but, since each must have lasted less than 40 days, a total of 100 days for the three invasions might be closer to reality. In this connection, the well-known remark that, before the occupation of Decelea, Spartan invasion of Attica caused only slight damage should be noted; *Hellenica Oxyrhynchia* 12.5. For a full discussion of this matter, see William G. Hardy, "The *Hellenica Oxyrhynchia* and the Devastation of Attica," *Classical Philology*, 21 (1926): 346–55.

¹⁰³ Archidamus led an army to Plataea in the summer (presumably May) of 429; Thucydides 2.71.1. After its arrival, some time was spent in negotiations, but, when these broke down, the Spartans began building mounds from which to attack the city's fortifications. According to Thucydides, the mound-building lasted seventy days. Gomme and others have dismissed this figure as unlikely; see, for example, Gomme, *A Historical Commentary on Thucydides*, 2: 207. It may well be too high, but, in any case, while part of the Peloponnesian army was discharged sometime during the summer of 429, not until the city had been surrounded by a wall "about the time of the rising of Arcturus"—that is, mid-September—was the main army finally withdrawn; Thucydides 2.78.1–2. Guards were left behind to continue the siege throughout the winter, and not until the following summer did the inhabitants finally surrender; Thucydides 3.20–24, 3.52–68.

expected to win the war quickly by invading Attica and, by extension, little or no support for modern arguments based upon that proposition. When Thucydides tells us that no one in Greece believed the Athenians could withstand more than three years of Spartan invasion of its territory or that the Spartans expected such invasions to end the war in a few years, he is making the most general of general statements. He did not, of course, know what everyone in Greece believed, nor was he privy, as his own lament testifies,¹⁰⁴ to the thinking of all Spartans. Such generalizations undoubtedly capture the essence of what the Greeks believed or the Spartans expected at any given moment, but they cannot be taken as literally true and universally applicable.¹⁰⁵ Above all, they cannot be accorded greater weight than the factual information Thucydides himself provides, and this information strongly suggests that Spartan strategy was considerably more complex than he realized.

¹⁰⁴ Thucydides 5.68.2. Also see Westlake, "The Naval Battle at Pylos and Its Consequences," 211–26.

¹⁰⁵ Anyone so inclined will have no difficulty locating similar general statements in Thucydides, along with proof of their general reliability and evidence against their universality. I cite just one example here: In 421, Thucydides tells us that "the Lacedaemonians . . . favoured peace because the war was turning out contrary to their expectations"; Thucydides (Loeb edn.) 5.14.3. The accuracy of the general statement needs no further proof than the conclusion of the Peace of Nicias shortly thereafter. Yet we know, from no less an authority than Thucydides himself, that two of the five ephors elected to office for the following year not only opposed the Peace of Nicias but actively sought to disrupt it; Thucydides 5.36.1. For a discussion of the activities of these ephors, see my "Cleobulus, Xenares, and Thucydides' Account of the Demolition of Panactum," *Historia*, 21 (1972): 159–69.

Patterns of Slaveholding in the Americas: New Evidence from Brazil

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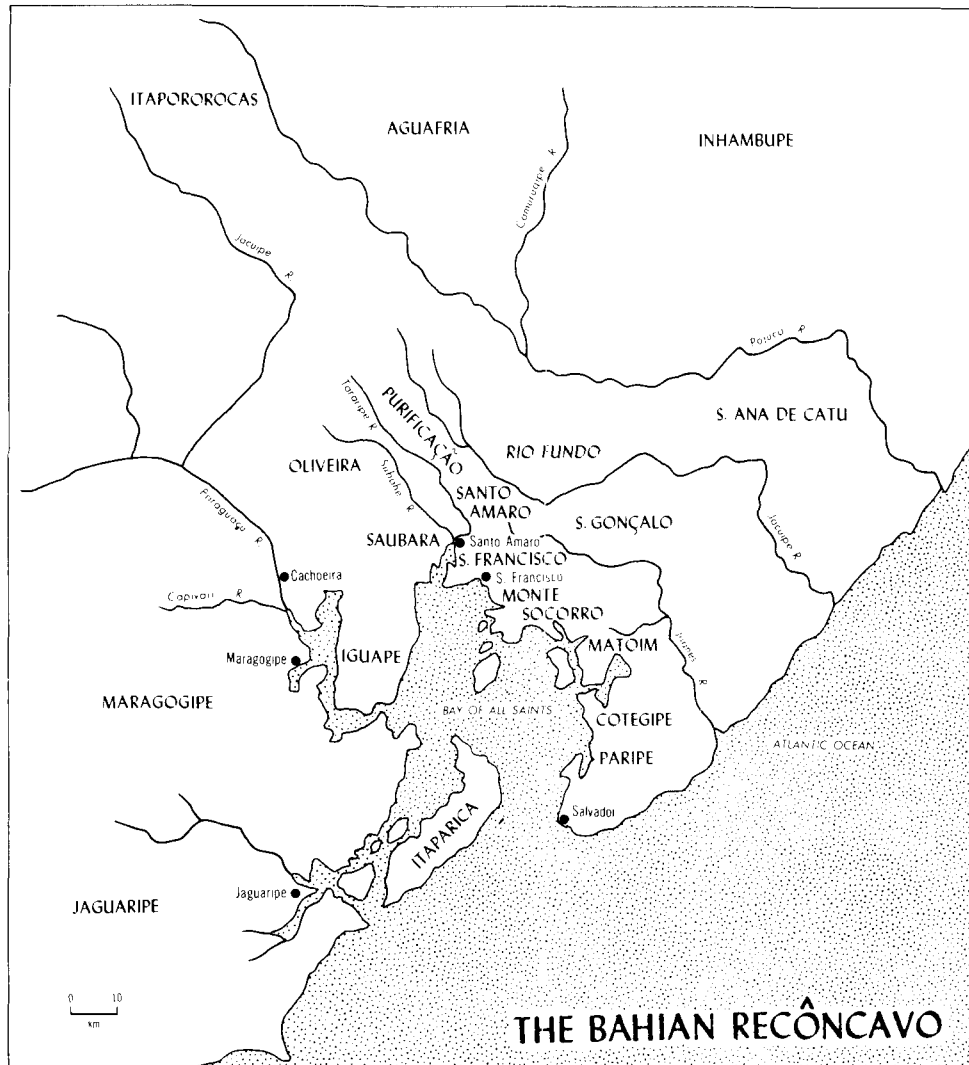
AS THE DEBATE ON ASPECTS OF SLAVERY has intensified and matured, certain shared assumptions have arisen about the various slave regimes, and these have then served as points of departure for the analysis of topics as varied as economic growth, the slave family, acculturation, and rebellion. Winthrop D. Jordan, in trying to isolate the factors that contributed to slave rebellions in the Americas, contrasted the situation in the United States to that in other areas. In a statement that few historians would dispute, he claimed that "plantation units in the South were far smaller than in the West Indies and Brazil."¹ The image of the West Indies and Brazil as regions of very large slaveholdings in comparison with the U.S. South has become hallowed in the historiography of comparative slavery; but that image is only partially accurate and somewhat misleading. Examining some newly available quantitative data from an important slaveowning region of Brazil permits not only placing this data into the context of Brazil as a whole, which entails paying particular attention to the complex arrangements of land and slaveownership that characterized its sugar plantation system, but also comparing more realistically the Brazilian situation to that in the U.S. South and in Jamaica.

The new evidence comes from one of the oldest and most important slaveholding regions of the Americas, the captaincy (province) of Bahia, especially from its agricultural heartland, the Recôncavo. This zone, encompassing a broad region of some ten thousand square kilometers circling the Bay of All Saints, was dominated by the city of Salvador, capital of the Brazilian colony from 1549 to 1763 and thereafter an important regional center.² From the middle of the sixteenth century,

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¹ Jordan, "Why Didn't Slaves Rebel?" Review of Eugene D. Genovese, *From Rebellion to Revolution: Afro-American Slave Revolts in the Making of the Modern World* (New York, 1980), in the *New York Review of Books*, April 17, 1980, pp. 18–20.

² For the best historical analysis of the relationship between the Recôncavo and the city of Salvador, see Kátia M. de Queirós Mattoso, *Bahia: A Cidade do Salvador e seu mercado no século XIX* (São Paulo, 1978). Also see F. W. O. Morton, "The Conservative Revolution of Independence: Economy, Society, and Politics in Bahia, 1790–1840" (D.Phil. thesis, Oxford University, 1974).



the Recôncavo had been a sugar plantation zone and a major terminus of the Atlantic slave trade. By the late seventeenth century, Bahia was the colony's largest exporter of sugar, and, although it suffered through hard times in the mid-eighteenth century as did the other plantation areas of Brazil, it continued to be the leader. In part this role was maintained by the development of other exports, such as tobacco. By the last decade of the eighteenth century, the Recôncavo of Bahia was still Brazil's most important zone of export agriculture and a region characterized by slavery.

Bahia, like the rest of plantation America, was swept along by the storm of revolutionary events in Europe and America. The elimination of St. Domingue in 1793 as a sugar-producer created new opportunities for other plantation regions. Bahian production increased in the 1790s, and along with it came increased levels of slave importation and worsening conditions of labor. Despite their failure, the

series of slave revolts that began in 1807 threw the government and the slaveowners into "terror and panic." In response to the servile threat, the governor of Bahia, the Count of Arcos, in September 1816 ordered that a census of slaveowners be made to determine the exact number of slaves, who held them, and where they were located. This accounting did little to help the government stem the tide of revolt, but the surviving lists do provide the historian with excellent material for examining the structure of slaveholding in Brazil.³ On these lists are recorded well over four thousand owners of 33,750 slaves in parishes representing almost all of the major physiographic and economic subareas of the Recôncavo. In 1819, Bahia had a slave population of about 147,000, or approximately 31 percent of the captaincy's almost five hundred thousand people.⁴ About one-half of Salvador's population of eighty thousand were slaves, and perhaps another 30,000 slaves lived in the parishes of Cachoeira, a tobacco-growing zone of the Recôncavo for which the lists have not survived.⁵ The remaining slaves lived scattered on ranches, farms, and mines in the interior, in parishes of mixed subsistence and export agriculture inland from the Recôncavo, or in the heavily forested zone of the Southern coast. The surviving lists thus record almost 23 percent of the captaincy's slaves and perhaps one-half of those who lived in the Recôncavo. In addition, the lists include the owners and slave forces of 165 *engenhos* ("mills" or sugar plantations) in the parishes of São Francisco and Santo Amaro townships, or over two-thirds of the Recôncavo's 240 *engenhos* and almost one-half of the captaincy's 340 plantations.⁶

³ The surviving lists are in APB, Cartas ao Governo, maços 232, 233, 234. Registration was carried out between September 1816 and January 1817. Instructions required that the lists include "all captives without distinction of color, sex, and age" and the names of the "masters to whom they belong and the properties that each of them owns." Notaries were required to testify that all slaveowners had reported fully and accurately. Lists were probably submitted from all of the Recôncavo parishes; but those of the suburban parishes of Paripe, Cotegipe, Pirajá, and Matoim have apparently not survived. A more serious gap is created by the absence of the returns from the township of Cachoeira and its parishes. This important zone had a population in 1819 of almost sixty-nine thousand and contained about thirty-four sugar plantations. More importantly, Cachoeira was the center of the Bahian tobacco economy. The lack of lists from Cachoeira and the failure of the recorders in the southern Recôncavo districts of Maragogipe and Jaguaripe to report owners' occupations prejudices any analysis of occupational ranking according to wealth in slaves. Extant lists do, however, include twelve of the Recôncavo's seventeen parishes within four of the six principal townships. Both urban and rural settings are represented, because the towns of Santo Amaro, Jaguaripe, and Maragogipe were recorded separately. Most importantly, all of the lists for the parishes of Santo Amaro and São Francisco, the heart of the sugar economy, survive.

⁴ Maria Luiza Marcilio, "Crescimento histórico da população brasileira até 1872," in *Crescimento populacional*, Cadernos de Centro Brasileiro de Análise e Planejamento (CEBRAP), no. 16 (São Paulo, 1974), 1–26. Also see Thomas Merrick and Douglas H. Graham, *Population and Economic Development in Brazil, 1800 to the Present* (Baltimore, 1979), 49–79.

⁵ In 1819, the parishes of Cachoeira had 12,523 households with seventy-seven thousand five hundred people and had forty-eight sugar mills or plantations, according to the German travelers J. B. von Spix and C. F. P. von Martius. Subtracting the area of Santo Amaro, which was included in their calculation but for which lists survive, the totals for Cachoeira are found to be as follows: 10,723 households, sixty-eight thousand seven hundred people, and thirty-four plantations. See Spix and Martius, *Viagem pelo Brasil*, 2 (2d Portuguese edn., São Paulo, 1976): 177.

⁶ There are 166 *engenhos* listed in the townships of Santo Amaro and São Francisco, but Engenho do Campo in Passé parish was inactive and thus has been eliminated from all calculations. In addition, although the lists from Maragogipe township did not identify property type, from the names of slaveowners it is possible to recognize 6 other *engenhos* so that the total number of *engenhos* appearing on the surviving lists is at least 172. All further calculations, however, are based on the 165 operating mills in the two major Recôncavo townships of Santo Amaro and São Francisco.

The *engenhos* of Santo Amaro and São Francisco were the biggest and best in the captaincy and should represent the upper limits of slaveholding in Bahia.⁷ And, if any slaveholding structure in Brazil was comparable to that on the Caribbean islands, slaveownership in these two townships should have been.

THE DISTRIBUTION OF SLAVEOWNERSHIP in Bahia was related to the land usage patterns of the region. As might be expected, the districts of the southern Recôncavo devoted primarily to subsistence farming or manioc production for local markets were characterized by small holdings, which is reflected in the mean number of slaves per holding and by the ratio of the proportion of slaveholders recorded to the proportion of slaves listed⁸ (see Table 1). The unit size of holding was notably smaller in Jaguaripe and Maragogipe than in the sugar-growing parishes of Santo Amaro and São Francisco, and, although the southern townships contained more than one-half of the owners listed (54.0 percent), these men and women held only slightly more than one-third of the slaves reported (34.1 percent). Jaguaripe provides an extreme example of a lack of concentration in slave property and a wide distribution of ownership.⁹ Almost three-quarters of the slaveowners in this region held fewer than five slaves and only twenty-five owners possessed more than twenty slaves. The largest units of ownership were those of the few *engenhos* of the region, but these were rather small in size and only three slaveowners held over fifty slaves.

Maragogipe was a district of transition from the forests and manioc farms of Jaguaripe to the tobacco and sugar zones of Cachoeira. Its economy was mixed, including some six sugar plantations, but determined largely by manioc and to a lesser extent by tobacco agriculture. Because the Maragogipe lists do not include owners' professions or occupations, it is difficult to evaluate the effect of tobacco planters on the structure of slaveholding, but the somewhat higher mean and

⁷ The parishes for which lists have been found are as follows: Vila de São Francisco; Nossa Senhora de Socorro, São Sebastião de Passé, Santa Anna do Catú, Nossa Senhora do Monte, São Gonçalo, Madre de Deus de Boqueirão (São Gonçalo and Boqueirão were listed together and are analyzed jointly in the discussion presented here); Vila de Santo Amaro; Nossa Senhora da Purificação, São Pedro de Rio Fundo, Nossa Senhora de Oliveira, São Domingos de Saubara; Vila de Jaguaripe; and Vila de Maragogipe. The lists from the last two were made by district rather than by parish. For a brief discussion of the Recôncavo towns, see Luís dos Santos Vilhena, *A Bahia no século XVIII*, 3 vols. (Salvador, 1969), 2: 475–86.

⁸ I have used slaveholdings as units of slave ownership, just as the lists record them. This makes sense from the slave's point of view, since these units formed the context in which they lived; but it does not resolve problems of multiple ownership—one individual holding two or more separate units. Data were not coded by the owners' names, so I have been unable to aggregate the holdings of persons with two or more slaveholdings. For the largest units, the sugar plantations, the problem is not acute, because notaries often listed all units held by a single planter together so that multiple holdings are made clear. In four instances, however, this causes the opposite problem because the number of slaves is given in total and not by unit. In these cases I have simply taken the average per plantation unit. Holdings in more than one parish present more of a problem, but there do not seem to have been many of these. One Bahian slaveowner held 500 slaves and at least three held over 300, but no single unit of ownership was composed of more than 237 slaves.

⁹ I have eliminated 5 owners from my calculations because the condition of the documents does not permit a reading of the number of slaves they held. The Bahian lists record 4,662 individual units of slaveownership that, because of four cases of joint recording, can be reduced to 4,653 "owners." When the 5 Jaguaripe owners whose slaves cannot be determined are eliminated, the total number of owners becomes 4,648, holding 33,750 slaves.

TABLE 1
Slaveholding in the Bahian Recôncavo, 1816–17

| <i>Parish</i> | <i>Number of Owners</i> | <i>Percentage of Total Owners</i> | <i>Number of Slaves</i> | <i>Percentage of Total Slaves</i> | <i>Mean Number of Slaves per Owner</i> | <i>Median Number of Slaves per Owner</i> |
|---------------|---------------------------------|---|---------------------------------|---|--|--|
| JAGUARIPE | 1,167 | 25 | 5,071 | 15 | 4.5 | 2.6 |
| MARAGOGIPE | 1,347 | 29 | 6,450 | 19 | 4.8 | 3.9 |
| SANTO AMARO | (1,363) | 29 | (12,168) | 36 | (8.9) | (3.3) |
| Purificação | 481 | | 4,807 | | 10.0 | 3.1 |
| Oliveira | 252 | | 1,303 | | 5.2 | 2.6 |
| Rio Fundo | 491 | | 5,178 | | 10.5 | 4.2 |
| Saubara | 139 | | 880 | | 6.3 | 2.5 |
| SÃO FRANCISCO | (776) | 17 | (10,061) | 30 | (12.9) | (4.3) |
| São Gonçalo | 271 | | 2,823 | | 10.4 | 3.6 |
| SOCORRO | 72 | | 854 | | 11.9 | 2.6 |
| Monte | 125 | | 2,448 | | 19.5 | 5.1 |
| Catú | 152 | | 1,420 | | 9.3 | 4.4 |
| Passé | 156 | | 2,516 | | 16.1 | 5.6 |
| TOTALS | 4,653 | 100 | 33,750 | 100 | 7.2 | |

median figures for Maragogipe in comparison with Jaguaripe must reflect the presence of tobacco planters. In addition, the Maragogipe district included two medium-sized sugar plantations, Engenho Sinunga of José Alexandre de Queirós with 80 slaves and Engenho Capanema of Jeronimo da Costa Almeida with 73 slaves, and four small mills, all of which probably drew on cane supplied by tenant or sharecropping *lavradores de cana*. These larger units associated with sugar production increase the average number of slaves per holding and disguise somewhat the degree to which Maragogipe was a region of small slaveholding—two-thirds of the owners held fewer than 5 slaves, although only 29 percent of the slaves lived on units of that size. Over 11,500 slaves lived in Maragogipe and Jaguaripe, demonstrating clearly that subsistence crop agriculture was not exclusively a free peasant activity in colonial Brazil—slave labor was consistently applied to this agriculture.

The parishes of the townships of Santo Amaro and São Francisco do Conde were the core of the Recôncavo's sugar economy and contained the largest *engenhos* and the greatest number of slaves. Not all of the parishes were alike in size or other characteristics. Saubara, for example, lay to the south of the Subaé and Sergi rivers, along which most of the Santo Amaro *engenhos* were located. Although Saubara had a few sugar plantations, it also produced large quantities of manioc and other food crops. The parishes of São Gonçalo and Rio Fundo, somewhat inland from the Bay of All Saints, had developed as sugar-growing areas in the mid-eighteenth century.

These parishes were, therefore, sugar districts still in formation and tended to exhibit a less concentrated ownership of slaves and a more diversified economy than the older seaside parishes of Nossa Senhora do Monte and Nossa Senhora da Purificação. In these traditional sugar parishes, the average size of holding was high, reaching almost 20 slaves per unit in Monte, a parish that contained twenty *engenhos*, three of which held over 150 slaves. The parishes within the townships of Santo Amaro and São Francisco do Conde contained less than one-half of the recorded slaveowners (46.0 percent), yet these masters controlled almost two-thirds of the slaves listed (65.9 percent). The strong association of sugar and slavery is certainly borne out in these figures.

Within this rural world of sugar, manioc, and tobacco, small villages and towns were scattered, usually on the banks or at the mouths of important rivers. Towns like Jaguaripe, Maragogipe, and the larger Santo Amaro were closely integrated into the countryside that surrounded them. They were places of commerce and administration rather than centers of production; and, except for a few stills that produced *cachaça*, the local rum, and a small number of artisan establishments, these towns lived off the activity of the agriculture that surrounded them. Slaveholding in this urban context differed considerably in scale and purpose from that in the countryside. Slaves in rural towns were employed as porters, dockmen, servants, and cooks as well as in other domestic and artisan occupations. The mean size of the urban slaveholdings was small, and the distribution of slaves among the owners was more equal than it was in rural areas.

In order to discuss the structure of Bahian slaveholding in a comparative framework, I have computed two statistical measures that are revealing of the pattern of distribution. The Gini coefficient is a measure of relative inequality or of dispersion—in this case, wealth in slaves—from a hypothetical situation of absolute equality. The Gini coefficient is expressed over a range from 0.0, or perfect equality, to 1.0, or absolute concentration. The higher the Gini coefficient, the more concentrated the slaveholding or the less equal the distribution between slaves and owners.¹⁰ As a statistical measure, the Gini index shows the distribution of wealth in slaves at a particular point in time and provides a measure for comparing distributions across time and place. In addition to the Gini coefficient, I have computed the size share in slaves held by the top 10 percent (SSTT) of owners. This simple measure, expressed as a percentage, is particularly sensitive to changes in the size of the largest slaveholdings.

The impact of specific economic activities on the distribution of slaves is apparent from these two measures and from the arithmetic mean size of the slaveholding units in the Recôncavo (see Table 2). Organization of the data according to location and type of economic activity, in ascending order of slave concentrations, clearly reveals the degree to which all of the lowest measures are associated with urban slavery. In rural areas the lowest indices are found in the zones of subsistence-crop

¹⁰ For an excellent description of the Gini coefficient, see Charles M. Dollar and Richard J. Jensen, *Historian's Guide to Statistics* (New York, 1971), 121–26. The formula for computing the Gini index is

$$G = 1 - 2 \sum_{i=1}^n P_i \text{Cum } Y_i + \sum_{i=1}^n P_i Y_i.$$

TABLE 2
Distribution of Slaveholding in Bahia

| <i>Location (Economic Setting)</i> | <i>Gini Coefficient of Inequality</i> | <i>Size Share Held by Top 10 Percent (SSTT) of Owners</i> | <i>Mean Number of Slaves per Owner</i> |
|---|---------------------------------------|---|--|
| RECÔNCAVO (Urban) | | | 4.0 |
| Maragogipe | .23 | — | |
| Jaguaripe | .34 | — | |
| Santo Amaro | .36 | — | |
| RECÔNCAVO (Manioc) | | | 4.5 |
| Jaguaripe | .38 | — | |
| Maragogipe | .45 | — | |
| SERTÃO (Cattle-Manioc), 1788 ^a | | | 5.2 |
| Inhambupe | .48 | — | |
| Agua Fria | | | |
| Taperagoa | | | |
| RECÔNCAVO (Mixed Sugar-Manioc) | | | 6.6 |
| Oliveira | .52 | 47% | |
| Catú | .55 | 43% | |
| Saubara | .59 | 56% | |
| RECÔNCAVO (Sugar) | | | 11.7 |
| Rio Fundo | .62 | 53% | |
| São Gonçalo | .64 | 54% | |
| Passé | .65 | 63% | |
| Purificação | .67 | 54% | |
| Socorro | .70 | 58% | |
| Monte | .77 | 78% | |

^a Slaves in the 1788 census were listed by the household to which they belonged rather than by owner. The Gini coefficient is calculated on households and the mean number of slaves is per household head. When slaves of all persons are included, the mean number rises to 5.7.

(manioc) agriculture in Jaguaripe and Maragogipe; the slightly higher measures for Maragogipe reflect the presence of some tobacco and sugar agriculture. The concentration of wealth in slaves and the mean size of holding increases from the manioc region to the areas of mixed manioc and sugar agriculture and finally reaches the highest levels in those parishes most thoroughly devoted to export sugar production. Within the traditional sugar parishes, all but one of the newer plantation parishes like Rio Fundo and São Gonçalo—further from the coast and on the outer edge of the Recôncavo—had a lower concentration of slave wealth than did the seashore parishes like Socorro and Monte. Table 2 includes data from three parishes in the Bahian interior taken from an unpublished census of 1788.¹¹

These parishes were primarily devoted to cattle ranching and subsistence-crop agriculture, and they represent an intermediate stage in the association of levels of concentration and size with specific kinds of economic activity.

Certainly the most striking aspects of Tables 1 and 2 are the relatively smooth distribution of slaveowning in the Bahian Recôncavo and the concomitant small size of an average holding. The Recôncavo was, despite its diversity, a major sugar plantation region, the leading Brazilian exporter of that crop in the early nineteenth century. As such, it should present the upper limits of inequality of wealth and resources traditionally associated with plantation zones. The relatively low concentrations of slave wealth in all areas except the seashore sugar parishes, the rather moderate concentrations even in the majority of these locations, and the quite low concentrations for the Recôncavo as a whole all suggest that our general conception of the structure of slave ownership in late colonial Brazil needs to be considerably modified.

Sugar production was an important activity elsewhere in the colony, but the plantations and mills in other regions were generally smaller than those in Bahia. A census of the plantations of Rio de Janeiro in 1778 lists 109 estates with an average of only thirty-six slaves and a median of thirty slaves per estate (for a Gini coefficient of 0.35). The *engenhos* of São Paulo were of comparable size.¹² Thus, the Bahian sugar plantations were the largest units and had the highest concentrations of slaves in the colony. Yet the majority of slaveowners were not active in sugar production but were, instead, involved in other economic activities or in urban settings, which had much lower concentrations of wealth in slaves. Slavery as an institution, an economic system, and a form of wealth was widely distributed among the Brazilian population. By the end of the colonial era, neither Brazil as a whole nor its Bahian Recôncavo was simply a sugar plantation writ large.

THE SLAVEOWNERS WERE AS VARIED AS THE TYPES OF SETTINGS in which slaves were held. Men, women, and religious institutions all held property in slaves. Men and women held slaves in proportionate ratios—that is, 80 percent of the owners were men, and men held 80 percent of the slaves. Subtracting the less than 1 percent of the slaves held by religious institutions, women as a group held a proportionate 20 percent share, but there was considerable variation among the female owners. The lists carefully record women who used the title *Dona*, an honorific term of prestige usually associated with wealth, respect, and, presumably, white skin. The average size of a holding for women without this title was four slaves, and for women with the *Dona*, twenty-three slaves. These figures reflect the fact that women with the title owned twenty-seven of the sugar-parish *engenhos*, which included some of the largest plantations in Bahia.

¹¹ APB, Colonia, MSS Recenseamentos. The bundle of these census returns was created in the Colonia section after I discovered them scattered throughout the Cartas ao Governo section.

¹² I have based the calculations for Rio de Janeiro on "Relações parciais apresentadas ao Marquez de Lavradio (1778)," *RIHGB*, 76 (1913): 289–360. For São Paulo, see Suely Robles Reis de Queiroz, "Algumas notas sobre a lavoura do açúcar em São Paulo no período colonial," *Anais do Museu Paulista*, 21 (1967): 109–277.

Feminine ownership of property reached its apex in Purificação, one of the oldest and most traditional of the sugar parishes. There, thirteen of the district's thirty-seven *engenhos* were owned by women, who in a number of cases held more than one plantation. Dona Maria Joaquina Pereira de Andrade, for example, owned the two former Jesuit estates of Pitinga (with 164 slaves) and Sergipe do Conde (with 237 slaves), which was the largest single slaveholding in the Recôncavo. In addition, she owned two other *engenhos* in the parish, Botelho (with 66 slaves) and Santa Catherina (with 87 slaves). To feed her bondsmen Dona Maria Joaquina owned three *fazendas* ("farms") in neighboring Saubara, which were worked by another 34 slaves. Holding a total of 588 slaves in two parishes, Dona Maria Joaquina was the largest single slaveowner in the Recôncavo. Her case is extraordinary, but she was by no means unique. Her neighbor, Dona Ana Joaquina de São José Aragão, owned two mills, Engenho São Miguel (with 64 slaves) and Engenho Rosario (with 62 slaves), and other instances of large-scale feminine ownership of slaves can be cited. In the townships of Santo Amaro and São Francisco, women owned about 16 percent of all of the *engenhos* and 10 percent of the cane farms.

The active participation of elite women in the control of land and slaves belies some of the common generalizations often made about the protected and submissive role of women in colonial Brazil. But there was an incongruity between the reality of these women's lives and the roles expected of them,¹³ a situation made clear in a scandalous lawsuit brought by a brother and sister against their mother in 1800. The children claimed that their mother, Dona Ana Joaquina Freire, had ruined their patrimony while acting as trustee of their father's estate, which included Engenho Boca do Rio in Paripe parish. The children accused their mother of carrying on an illicit affair with a nearby cane farmer and favoring him to the detriment of the *engenho*. What is most interesting are the depositions of various witnesses, who testified that she had managed the *engenho* even when her husband was alive but that "administration by a woman, even if she is active and efficient, is, as a rule, not to the best advantage."¹⁴ Whatever the reality of economic activity among elite women, societal attitudes toward such roles remained negative.

Although elite women who controlled large *engenhos* are colorful examples of feminine power and activity, women made up only 20 percent of the property owners and as a group tended to own smaller slaveholdings than did men. Three-quarters of the women who owned slaves held fewer than five while only one-half of the men fall into that category. Small-scale and probably domestic slaveowning by women was especially noticeable in urban areas like the town of Santo Amaro, where 30 percent of the slaves were held by women.

Both individual clerics and religious institutions were also counted among the Recôncavo's slaveholders. By 1817 the former Jesuit estates were in secular hands, but the Benedictines and the Carmelites owned *engenhos* and cane lands. The

¹³ A. J. R. Russell-Wood has provided, in a different context, a brief discussion of inheritance practices; see Russell-Wood, "Women and Society in Colonial Brazil," *Journal of Latin American Studies*, 9 (1977-78): 1-34.

¹⁴ APB, Cartas ao Governo, Maço 209 (23 September 1800).

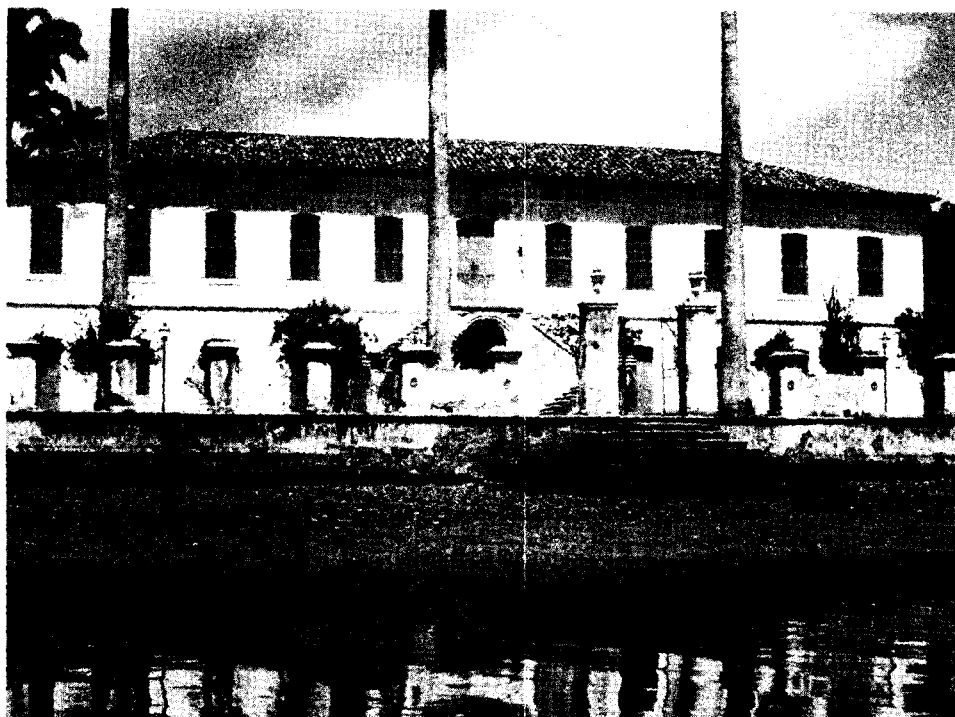


Figure 1: Engenho Cajaíba in the township of São Francisco is located on an island in the Bay of All Saints. The "Big House" demonstrates the grandeur of some of the larger plantations. Photograph taken by the author in 1974.

Benedictines owned São Bento de Lages, a beautiful *engenho* destined to become Bahia's first school of agriculture, in São Gonçalo and Inhatá, a smaller mill, in Rio Fundo, the Carmelites owned Engenho do Carmo, located in Passé. The individual clerics who owned slaves were very much a part of the society in which they lived, and little distinguished them from the secular slaveowners. In the towns, priests held small numbers of slaves as servants, butlers, and cooks, but in the countryside clerics employed slave labor in agricultural pursuits. As sons of the rural elite, some priests acquired or inherited *engenhos* and cane farms. Six priests were listed as *senhores de engenho* ("plantation owners") and another thirty as *lavradores de cana* ("cane farmers"). In a number of cases arrangements existed that permitted priests to perform their clerical functions while participating in the sugar economy. Such was the situation of Father Rafael de Sousa Gomes, who served as chaplain at Engenho Retiro and employed nine slaves in growing cane for that mill.

The lists for Santo Amaro and São Francisco townships record considerable occupational information for the 2,152 slaveowners and permit the ranking of these slaveholders according to their productive or economic sector¹⁵ (see Table 3). Such a ranking contains few surprises, although what is missing is as interesting as

¹⁵ Colin Clark, *The Conditions of Economic Progress* (3d edn., New York, 1960), 490–92. Clark has defined the primary sector of economic activity as that which provides the products of agriculture and the sea and which depends on the direct use of natural resources. The secondary sector does produce moveable goods on a continuous basis through manufacture of craft. The tertiary sector includes transport, communication, commerce, finance, professional services, and domestic employment, among quite a wide range of services. I have followed the application of these definitions to the Brazilian situation as outlined in Maria Luiza Marcílio, *A Cidade de São Paulo: Povoamento e população, 1750–1850* (São Paulo, 1968), 129–35.

what is present.¹⁶ In the primary sector of agriculture, the ownership or control of property most closely linked to the export economy was the crucial element determining the size of the slave force. This may, however, be a somewhat circular argument, since certain kinds of agriculture demanded specific levels of labor input. The *senhores de engenho* held the largest slave forces and stood at the top in terms of wealth in slaves, just as they did in social prestige. Among their number were seven men who rented rather than owned plantations and whose holdings in slaves were considerably smaller than those of the plantation owners as a whole. No renter owned more than fifty-eight slaves, and the mean size of a holding for the renters was only thirty-seven slaves, compared with sixty-six for the *senhores de engenho* as a whole. Operation of a mill was not, in and of itself, an explanation of the level of wealth in slaves, and the ownership of land seems to have been an important factor in wealth accumulation.

Given the importance of the sugar economy in Bahia and the traditional position of the *lavradores de cana* as proto-planters, cane farmers might be expected to fall just below the *senhores de engenho*, but instead they follow the tobacco farmers and other farm owners. This anomaly is probably the result of the lack of extant lists from Cachoeira, center of the tobacco economy, and the growing prominence of other farm owners, some of whom produced some sugar cane. Of the four tobacco growers listed for Santo Amaro and São Francisco townships, three owned fewer than nine slaves, but one held sixty bondsmen, which pushes the mean size of holding to unexpectedly high levels. In general, a large tobacco farm in the early nineteenth century might have had twenty-five slaves, and many had only two or three. Using notarial contracts for a century earlier, Rae Flory has found that, at that time, tobacco farms were, on average, one-third the value of cane farms.¹⁷ The other types of agricultural property commonly found in the Recôncavo were the *fazenda* and the *sítio*,¹⁸ both of which roughly translate into English as "farm." The distinctions between them are difficult to ascertain. *Fazendas* could be devoted to a variety of activities ranging from cattle to food crops and occasionally sugar cane. *Sítio* was the term preferred for tobacco farms but was not exclusively used in this way. Size, crop type, tradition, and probably personal preference all influenced the designation of a property as a *fazenda* or a *sítio*. For convenience, I have combined the fifty-one owners of *fazendas* and the twelve proprietors of *sítios*. Together the mean size of the holdings is just over thirteen slaves. Those who rented *fazendas* or pieces of them were poorer than both *fazenda* owners and *lavradores de cana* and

¹⁶ Instructions for making the lists required that the names and properties of the masters be listed but were not clear about the recording of the masters' professions. In some parishes, like Maragogipe and Jaguaripe, no attempt was made to record either property or occupation. In Oliveira, the list was organized around rural properties, *engenhos* and *fazendas* ("farms"), so that by inference the occupation of the owners of these properties can be deduced; but many persons in this parish remain without occupational information. Despite such gaps and problems, if we view slave property as a form of wealth, then it is possible to rank occupations according to that wealth and, by doing so, to suggest the relative power of classes and groups in Bahian society.

¹⁷ Flory, "Bahian Society in the Mid-Colonial Period: The Sugar Planters, Tobacco Growers, Merchants, and Artisans of Salvador and the Recôncavo, 1680–1725" (Ph.D. dissertation, University of Texas, Austin, 1978), 182.

¹⁸ *Sítio* was a term also used simply to mean "place."

TABLE 3
Ranking of Occupations according to Wealth in Slaves

| <i>Occupational Activity</i> | <i>Mean Number of Slaves per Owner</i> | <i>Number of Owners</i> | <i>Percentage of Total Owners</i> |
|--------------------------------------|--|---------------------------------|---|
| PRIMARY ACTIVITIES | | | |
| <i>Senhor de engenho</i> | 65.5 | 165 | 7.7 |
| Tobacco farmer | 19.3 | 4 | 0.2 |
| <i>Fazenda</i> or <i>sítio</i> owner | 13.4 | 63 | 2.9 |
| <i>Lavrador de canas</i> | 10.5 | 478 | 22.2 |
| Manioc farmer | 6.2 | 128 | 5.9 |
| <i>Lavrador de cal</i> | 5.9 | 42 | 2.0 |
| <i>Fazenda</i> renter | 5.2 | 125 | 5.8 |
| <i>Agregado</i> | 2.8 | 85 | 3.9 |
| <i>Morador</i> | 2.4 | 25 | 1.2 |
| Fisherman | 1.7 | 3 | 0.1 |
| SECONDARY ACTIVITIES | | | |
| Distillery owner | 12.1 | 23 | 1.1 |
| Artisan | 2.4 | 18 | 0.8 |
| TERTIARY ACTIVITIES | | | |
| Warehouse owner | 6.0 | 2 | 0.1 |
| Priest | 4.5 | 19 | 0.9 |
| Home owner | 4.2 | 41 | 1.9 |
| Bureaucrat or professional | 2.2 | 11 | 0.5 |
| Small businessman | 2.4 | 64 | 3.0 |
| <i>Suas agencias</i> | 1.9 | 30 | 1.4 |
| Overseer | 1.5 | 2 | 0.1 |
| NO OCCUPATION LISTED | 3.4 | 824 | 38.3 |
| TOTAL | | 2,152 | 100.0 |

NOTE: The *senhor de engenho* figures represent single units of ownership (*engenhos*). There were in 1816–17 four cases of multiple ownership encompassing thirteen units.

held only about one-third to one-half of the number of slaves. Thus, ranking the owners and tenants of agricultural properties by the size of their slaveholdings tends to confirm the impression created by travelers to Pernambuco, who described the decline of the *lavrador de cana* class in wealth and prestige,¹⁹ for a similar process seems to have been taking place in the Bahian Recôncavo. The *lavradores de cana* were still a substantial class of rural slaveholders, but those who owned or controlled land in *fazendas* and *sítios* apparently were, as a group, wealthier. Moreover,

¹⁹ For a discussion of the decline in prestige of the *lavradores de cana*, see my "Elite Politics and the Growth of a Peasantry in Late Colonial Brazil," in A. J. R. Russell-Wood, ed., *From Colony to Nation: Essays on the Independence of Brazil* (Baltimore, 1975), 133–54.

evidence from the census of 1788 suggests that *lavradores de cana* increasingly included people of color among their ranks, a situation that in Brazil was a sure sign of a decline in social prestige.²⁰

On a different scale of wealth were those slaveowners who were not directly connected to the major export crop or whose limited access to land and capital kept their operations small. The manioc farmers who owned their own plots averaged slightly over five slaves a unit as did the renters of *fazenda* land. In the parish of Boqueirão, a number of individuals made a living by preparing *cal* ("lime"), a material essential for building construction. At the bottom of the agricultural slaveholders were the dependents of the *engenhos* and *fazendas*. Often people of color, freedmen and their descendants, this segment of the population had begun to increase in the period after 1750.²¹ Contemporary observers in the early nineteenth century always described these people as the poorest of the free rural population, so that those listed here as slaveowners must surely be somewhat wealthier than the majority of their class. This group falls into two categories, *agregados* and *moradores*. *Agregados* were individuals who were associated with a rural property or, in fact, any household—living as part of it—yet who maintained their own families and properties. Often they were boarders, distant relatives, or simply employees accepted within the household. Even less advantaged were the *moradores* who were allowed to farm some marginal land in return for occasional labor or other services. In 1817 this group probably constituted about 20 percent of the rural population of Pernambuco. If a similar situation existed in Bahia, then the presence of only twenty-five *moradores* among the slaveholders is an indication of their poverty, not their numbers.

The secondary economic sector includes those activities associated with manufacturing or with artisanal crafts. In the Recôncavo lists, the number of individuals with these occupations is very small. There are several possible explanations for this situation, some structural and some accidental. It may have been the case that some notaries listed only "properties" in the sense of land or possessions and did not bother to include occupation. There were 824 slaveowners listed without occupational designation, and included among them may have been some artisans. A second possible explanation is that artisans did not, in general, own slaves, but this seems unlikely. Perhaps most plausible is an explanation that emphasizes the impact of slavery itself on the artisan trades. In the Recôncavo, many of the traditional artisan skills were practiced by the slaves. *Engenhos* often found it more profitable to train slaves as carpenters, smiths, or coopers rather than to pay for the services of free artisans. Hence, the relatively small number of artisans listed may be the result of an erosion in the size and social position of that class in the face of a slave regime. Santos Vilhena noted in 1798 that blacks filled all of the servile and mechanic arts and rarely did a mulatto, much less a white, perform these tasks.²² The only manufacturing done in Santo Amaro and São Francisco townships was

²⁰ In the parish of Rio Fundo in 1788, there were thirteen heads of household listed as *lavradores de cana*, of whom four were people of color. See APB, Colônia, MSS Recenseamentos.

²¹ Schwartz, "Elite Politics and the Growth of a Peasantry," 150–52.

²² Vilhena, *A Bahia no século XVIII*, 1: 137–38.

the distilling of *cachaça*, the local rum. The *alambiques* ("stills") varied in size from household enterprises with four to five slaves up to factories with twenty to forty enslaved workers.

The tertiary sector, that of "service" occupations, actually includes a wide range of professions and activities, many of which were associated with the towns and villages of the Recôncavo. The two *trapiche* ("warehouse") owners obviously provided a necessary service for the sugar planters. But the tertiary sector here also encompasses those who were listed simply as homeowners in the towns and for whom slaveowning was primarily a form of domestic employment. The same can probably be said of the priests who served the Recôncavo. Included in the bureaucrats and professionals are lawyers, notaries, a doctor, tax collectors, teachers, and minor officials. Perhaps of most interest in the tertiary sector are those involved in small-scale commerce: shopkeepers, *mascales* ("traveling peddlers"), and individuals who simply lived *de negocios* ("by business"). All of those listed were from the parishes of São Francisco township, which suggests that the officials responsible for recording the parishes of Santo Amaro simply did not bother to note this form of occupation. Some individuals who were listed as living by *suas agencias* ("their activities") probably rented out their slaves or put them to work in the towns as market women or porters and lived off their earnings. Later in the century, Thomas Ewbank noted that, in Brazil, "hundreds and hundreds of families have one or two slaves on whose earnings alone they live."²³

IT WAS NOT THE SLAVEOWNERS with one or two slaves who set the patterns of the society and economy in the Recôncavo but the *senhores de engenho*, the planters, who for two and a half centuries had dominated Bahian life. The 1816–17 lists offer an excellent opportunity to examine the slaveholding of that class in a quantitative way and to analyze the organization of the plantation economy. The lists record the name and size of the slave force of 165 *engenhos*, or about three-quarters of the plantations in the Recôncavo and about one-half of the sugar mills in the captaincy. By 1817, the effects of the sugar industry's expansion in the wake of the Haitian Revolution and the Napoleonic wars could be seen in the increased number of mills. By that date, Bahia had about 316 *engenhos*, an increase of about 100 plantations since 1790.²⁴ About twenty interconnected traditional families—such as the Calmon, Fiuza Barreto, Costa Pinto, and Pires de Carvalho—controlled about one-third of the *engenhos*. These "aristocratic" families were especially dominant in the coastal parishes and on lands less than ten miles from the shore. Often within this group there were instances of ownership of more than one plantation. They were particularly well represented in the lists from Santo Amaro and São Francisco do Conde.²⁵ Since the largest mills were concentrated in these townships, the 165 *engenhos* are an excellent and large sample of the Bahian plantations—a sample

²³ Ewbank, *Life in Brazil: or, A Journal of a Visit to the Land of the Cocoa and the Palm* (New York, 1856), 184.

²⁴ "Quadro dos engenhos das vizinhanças da Bahia" lists 354 *engenhos* in Bahia and Sergipe; BNJR, I–7, 3, 27. Also see the calculations in Morton, "The Conservative Revolution of Independence," 13–19.

²⁵ Morton, "The Conservative Revolution for Independence," 20–29.

that, if it has any particular bias, probably tends to exaggerate the average size of an *engenho* slave force.

Two features of the Recôncavo *engenhos* immediately stand out. First, there was a relatively smooth distribution in the size of the slave force among the *engenhos*—that is, there were few very small or very large units (see Table 4). The most common size ranged between sixty and one hundred slaves, but a considerable number of plantations operated with twenty to sixty workers (and the Gini coefficient of slaveownership among the *senhores de engenho* is low). Second, the 165 *engenhos* listed averaged only sixty-five slaves per *engenho*, if only those slaves who were owned directly by the mills are counted. Estates with a slave force of this size do not seem to fit the common image of the Brazilian plantation as a great latifundium with hundreds of slaves laboring under the unitary and patriarchal hand of a resident planter-lord. In fact, only Engenho Sergipe do Conde had a slave force of over two hundred bondsmen, and only about 15 percent of the mills counted one hundred slaves or more.

The crucial point here is that the unit of production, the *engenho*, drew not only on the labor force of its owner but also on the slaves of the *lavradores de cana* and sometimes on those of the other tenants and employees. The listed number of slaves for a *senhor de engenho* does not fully disclose the size of the labor force associated with the mill, and, when all of the slaves are considered,²⁶ then the sizes of the “great” plantations are approached. Since ownership of the labor force was divided, slaves lived in smaller groups than historians have heretofore thought, which probably had considerable impact on the lives of both masters and slaves.

Although it is possible to speak of an average number of slaves on a Bahian *engenho*, it is more useful to view plantation scale in terms of optimal sizes. Over 80 percent of the Bahian *engenhos* had between twenty and one hundred slaves. Large mills with direct control over more than one hundred slaves were concentrated along the coast. They were few in number and controlled less than 30 percent of the plantation slaves. If those *engenhos* with fewer than twenty slaves—presumably plantations in the process of formation or decay—are discounted, then small mills can be defined as those with twenty to fifty-nine slaves, medium-sized *engenhos* are those with sixty to ninety-nine hands, and large plantations those with over one hundred, of which mills with one hundred and fifty or more bondsmen were a small subcategory. These great *engenhos* came the closest to the romantic image of the colonial sugar plantation, but they were atypical, forming only a small fraction of all *engenhos* and holding about 17 percent of the plantation slaves. Neither the typical planter nor the typical slave lived on the great plantations of colonial Brazil. And these dimensions of slaveownership and plantation size were quite unlike those of other sugar plantation zones in the Americas. In Jamaica, for example, over one-half of the slaves on the island in 1832 lived on units of one hundred and fifty or more.²⁷

The sugar plantation structure in Bahia was a complex arrangement in which the

²⁶ For calculations on this point, see pages 71–73, below.

²⁷ B. W. Higman, *Slave Population and Economy in Jamaica, 1807–1834* (Cambridge, 1976), 274–75.

TABLE 4
Distribution of Slave Ownership
among *Engenhos* in Parishes of the Recôncavo, 1816–17

| <i>Size of Holding in Slaves</i> | <i>Number of Owners</i> | <i>Percentage of Total Owners</i> | <i>Number of Slaves</i> | <i>Percentage of Total Slaves</i> | <i>Gini Coefficient of Inequality</i> |
|--|-------------------------|-----------------------------------|-------------------------|-----------------------------------|---------------------------------------|
| <i>São Francisco Township (São Gonçalo, Catú, Passé, Socorro, Monte)</i> | | | | | |
| 5–9 | 0 | 0.0 | 0 | 0.0 | |
| 10–19 | 0 | 0.0 | 0 | 0.0 | |
| 20–39 | 14 | 17.5 | 408 | 7.4 | |
| 40–59 | 18 | 22.5 | 862 | 15.5 | |
| 60–99 | 34 | 42.5 | 2,759 | 49.6 | |
| 100–149 | 14 | 17.5 | 1,531 | 27.5 | |
| 150 or more | 0 | 0.0 | 0 | 0.0 | |
| TOTALS | 80 | 100.0 | 5,560 | 100.0 | .21 |
| <i>Santo Amaro Township (Rio Fundo, Purificação, Saubara, Oliveira)</i> | | | | | |
| 5–9 | 2 | 2.4 | 14 | 0.2 | |
| 10–19 | 3 | 3.5 | 49 | 0.9 | |
| 20–39 | 24 | 28.2 | 745 | 14.2 | |
| 40–59 | 14 | 16.5 | 687 | 13.1 | |
| 60–99 | 31 | 36.5 | 2,231 | 42.5 | |
| 100–149 | 7 | 8.2 | 787 | 15.0 | |
| 150 or more | 4 | 4.7 | 740 | 14.1 | |
| TOTALS | 85 | 100.0 | 5,253 | 100.0 | .30 |

NOTE: The total number of *engenhos* does not include one that was *fogo morto* (“inoperative”) in the parish of Passé in São Francisco Township.

engenhos were the central and crucial units of production, serving as the industry’s core but dependent on the contributions of the free rural population that lived within their shadows. This free population entered into a variety of contracts and arrangements with the *engenhos*, providing them with supplementary slave forces, labor and management, and an enlarged capital credit base for the industry. This non-mill-owning population of the *lavradores de cana*, *agregados*, *moradores*, and renters who often owned property in slaves and rarely in land, characterized the Recôncavo. Labor as a factor of production was not so concentrated in the hands of the great planters as in other regions of the New World; thus, the risks of sugar agriculture were more widely shared, which meant that the brunt of a natural disaster or of falling prices was not borne solely by the planters. The existence of these groups of free people who participated in significant numbers in sugar agriculture made the organization of that activity more complex than it was in other regions of the Americas.

The key groups within the structure of Bahian sugar agriculture were the tenants, sharecroppers, and small owners who grew cane for the *engenhos* and were thus entitled to the name *lavradores de cana*.²⁸ By 1817, there were between three and four *lavradores de cana* for each *engenho*, although some *engenhos* depended on the cane from ten or more *lavradores* and a few had none. These cane farmers owned their own slaves and livestock and also held about one-third of all of the slaves directly employed in sugar agriculture. *Lavrador* slaveholdings varied widely in size but had a relatively low level of concentration of slaves (see Table 5). Approximately one-quarter (24.5 percent) of the cane farmers were marginal producers with fewer than five slaves, and almost six-tenths (57.1 percent) held fewer than ten workers.

The disparity between *lavradores de cana* who held one or two slaves and those who held forty reflects the wide range of social and economic variation within this group and suggests reasons why care must be used in treating them as a homogeneous class. Earlier research has provided some idea of the size of *lavrador* slaveholdings, but the 1816–17 lists offer for the first time an opportunity to examine the distribution of slaveownership within this group in a comprehensive manner.²⁹ In the townships of Santo Amaro and São Francisco do Conde there were 478 individuals who can positively be identified as *lavradores de cana*. They owned 5,010 slaves for a mean size of slaveholding of 10.5. In both Santo Amaro and São Francisco do Conde just over 60 percent (61.7) of the *lavrador* owners held slaves in groups of 5 to 19, and slightly less than 60 percent (57.4) of the slaves held by *lavradores de cana* in the two townships lived in units of this size. This situation made slaveholding patterns in Bahia quite dissimilar to those in other sugar plantation areas. In Jamaica, for example, where the small- and medium-sized slaveholdings had been eliminated in the mid-eighteenth century, only 11 percent of the slaves lived in units of 5 to 20 at the time of registration in 1832. In Louisiana in 1850, under 20 percent of the sugar plantation slaves lived on estates of 50 or fewer. Even in Trinidad in 1814, with its more diversified economy not so fully committed to sugar, over 60 percent of the agricultural slaves lived in groups of 60 or more, and only 17 percent of the slaves lived in units of under 20 in size.³⁰ In contrast, 53 percent of all of the Recôncavo's slaves lived in groups of 1 to 20. This slaveholding structure reflects both the agricultural diversity of the region and the role of the cane farmers within the sugar economy.

The existence of the *lavradores de cana* as slaveowners with medium-sized holdings whose labor forces were committed to the sugar industry explains the

²⁸ Stuart B. Schwartz, "Free Labor in a Slave Economy: The *Lavradores de Cana* of Colonial Bahia," in Dauril Alden, ed., *Colonial Roots of Modern Brazil* (Berkeley and Los Angeles, 1973), 147–97.

²⁹ *Ibid.*, 173; and Flory, "Bahian Society in the Mid-Colonial Period," 30–45. Both studies offer estimates of land and slaveownership among the *lavradores de cana* based on notarial documents, and both offer correct estimates for the range of unit size; but, until now, it was impossible to determine the distribution of unit size among the *lavradores de cana*, so that some calculation of "typicality" and concentration could be made.

³⁰ Higman, *Slave Population and Economy in Jamaica*, 174–75; Frank Wesley Pitman, *The Development of the British West Indies, 1700–1763* (2d edn., reprinted, New Haven, 1963), 108–27; and Mark Schmitz, *Economic Analysis of Antebellum Sugar Plantations in Louisiana* (New York, 1977), 127–28. Information on Trinidad was kindly supplied by Professor Stanley L. Engerman.

TABLE 5
Distribution of Slave Ownership
among *Lavradores de Cana* in Parishes of the Recôncavo, 1816–17

| <i>Size of Holding in Slaves</i> | <i>Number of Owners</i> | <i>Percentage of Total Owners</i> | <i>Number of Slaves</i> | <i>Percentage of Total Slaves</i> | <i>Gini Coefficient of Inequality</i> |
|--|-------------------------|-----------------------------------|-------------------------|-----------------------------------|---------------------------------------|
| <i>São Francisco Township (São Gonçalo, Catú, Passé, Socorro, Monte)</i> | | | | | |
| 1–4 | 69 | 25.4 | 211 | 7.6 | |
| 5–9 | 90 | 33.2 | 590 | 21.3 | |
| 10–19 | 75 | 27.7 | 977 | 35.2 | |
| 20–39 | 36 | 13.3 | 944 | 34.1 | |
| 40–59 | 1 | 0.4 | 51 | 1.8 | |
| TOTALS | 271 | 100.0 | 2,773 | 100.0 | .37 |
| <i>Santo Amaro Township (Rio Fundo, Purificação, Saubara, Oliveira)</i> | | | | | |
| 1–4 | 48 | 23.2 | 152 | 6.8 | |
| 5–9 | 66 | 31.9 | 435 | 19.4 | |
| 10–19 | 64 | 30.9 | 875 | 39.1 | |
| 20–39 | 27 | 13.0 | 695 | 31.1 | |
| 40–59 | 2 | 1.0 | 80 | 3.6 | |
| TOTALS | 207 | 100.0 | 2,237 | 100.0 | .37 |

NOTE: In the parish of Oliveira, occupations were not recorded. There were probably twenty-one *lavradores de cana* who owned 133 slaves (or about 5.6 percent of the slaves held by *lavradores* in Santo Amaro Township), but the table above has been calculated without these *lavradores* and their slaves.

curious and distinctive structure of the Bahian (and, by implication, the Brazilian) sugar industry. The 165 *engenhos* of the Recôncavo depended on an average of three *lavradores* for each mill, and these *lavradores* owned an average of ten to eleven slaves each. Thus, about one-third of the slaves directly employed in sugar agriculture were owned not by the *engenhos* but by the cane farmers.³¹ This division of ownership is essential to understanding the scale of plantation operations in Bahia. If the *engenho* slave forces include not merely the slaves held by the *senhores de engenho* but also those held by the *lavradores de cana* (see Table 6), then the mean number of slaves per mill rises to ninety-six, a figure much more in line with the one hundred slaves estimated by various observers in the seventeenth and

³¹ The 1816–17 lists may be revealing of a process of concentration within the sugar industry. Flory suggested that each *engenho* required an average of fifteen cane farms for sugar production in the Recôncavo during the period 1680–1725; “Bahian Society in the Mid-Colonial Period,” 31–35. This number would include both tenants and those *lavradores* who owned their own land. If Flory is correct, then the percentage of the total slave force controlled by *lavradores* was probably higher in the early eighteenth century than at the start of the nineteenth. If she is correct, moreover, then there appears to have been a process of contraction and centralization that diminished the number of *lavradores de cana* in the Recôncavo, since the average number of *lavradores* declined from her estimate of fifteen per *engenho* to the one presented here of between three and four.

eighteenth centuries.³² Apparently contradictory statements, like that of the town council of Salvador in 1751 placing the average number of slaves on an *engenho* at forty, now also make sense, because of the implicit assumption that additional labor will be supplied by the *lavradores de cana* through their slaves.³³

Finally, it must be pointed out that the arrangements between *lavradores de cana* and *engenhos* reveal only a part, albeit a major one, of the complexity of plantation labor structures. Also resident on the lands or at the boundaries of many plantations were other free people who provided labor services themselves or whose slaves could be used by the *engenhos* at certain times of the year. It is difficult to know to what extent these slaves were employed regularly in sugar agriculture, and I have not included them in my calculations of *engenho* size to this point. In some parishes, however, slaveowners were recorded by their residence in relation to *engenhos*; thus, it is possible to see the relationship between plantation owners and other resident slaveowners. In Rio Fundo parish the thirty-eight *engenhos* averaged four *lavradores* each as well as two *agregados* each and one other resident slaveowner. This group of "other" slaveowners was made up of *moradores*, artisans, and managers. Although the mean size of an *engenho's* slave force in Rio Fundo was 59 if only those slaves owned directly by the plantation are counted, the figure rises to 112 when the slaves of all associated owners are included.

When "reconstructed" by aggregating the slaves of all of the dependents as part of the *engenho* work force, the scale of the Bahian plantations falls more closely in line with the size of sugar plantations found in the Caribbean and the southern United States. The complex arrangements of slaveownership and its diffusion within the sugar economy had important implications for both masters and slaves. With small-scale slaveholding a predominant feature in Bahia and medium-sized holdings common in the sugar economy, we need to re-examine our long-held notions about master-slave relationships and the basis of pervasive patriarchal attitudes held by many slaveowners. Within the slave communities, marital arrangements must have been difficult when the units of slaveholding were small, unless permission to marry outside the group of slaves held by one master was readily given; if normally granted, then extended family ties must have been tenuous, with various family members under different owners. And, for the planters, a major concern had to be the coordination of the *engenho's* schedule with the use of the existing labor force, much of which was under the control of others; management skill became crucial to a mill's success. Little wonder, then, that many Bahian planters were reluctant to leave the big house and to allow stewards to direct operations.

THE RELATIVELY LOW LEVELS OF CONCENTRATION of slaves in large units, the predominance of small- and medium-sized slaveholdings, and the apparently broad

³² For two such estimates, see "Discurso . . . descrição económica da Comarca e Cidade do Salvador," in Pinto de Aguiar, ed., *Aspectos da economia colonial* (Salvador, 1957), 36–37; and Dirk Ruiters, "A torcha de navegação," *RIHGB*, 269 (October–December 1965): 82–83.

³³ Arquivo Histórico Ultramarino (Lisbon), Bahia papeis avulsos (1751), box 61.

TABLE 6
Slaveholding by *Engenhos* and *Lavradores de Cana* among Parishes of the Recôncavo, 1816-17

| Parish | Number of <i>Engenhos</i> | Number of Slaves Held by <i>Engenhos</i> | Percentage of Slaves Held by <i>Engenhos</i> | Number of <i>Lavradores</i> | Number of Slaves Held by <i>Lavradores</i> | Percentage of Slaves Held by <i>Lavradores</i> | <i>Lavradores</i> per <i>Engenho</i> |
|-----------------------|---------------------------------|--|--|-----------------------------------|--|--|--|
| SÃO FRANCISCO | | | | | | | |
| São Gonçalo | 19 | 1,298 | 62.0 | 62 | 794 | 38.0 | 3.3 |
| Catú | 10 | 482 | 58.8 | 41 | 338 | 41.2 | 4.1 |
| Monte | 20 | 1,732 | 74.7 | 59 | 588 | 25.3 | 3.0 |
| Socorro | 9 | 562 | 77.0 | 13 | 168 | 23.0 | 1.4 |
| Passé | 22 ^a | 1,486 | 62.7 | 96 | 885 | 37.3 | 4.4 |
| SUBTOTALS | 80 | 5,560 | 66.7 | 271 | 2,773 | 33.3 | 3.4 |
| SANTO AMARO | | | | | | | |
| Purificação | 38 | 2,560 | 80.5 | 54 | 622 | 19.5 | 1.4 |
| Rio Fundo | 38 | 2,245 | 60.4 | 143 | 1,474 | 39.6 | 3.8 |
| Oliveira ^b | 6 | 239 | 100.0 | 0 | — | 0.0 | — |
| Saúbara | 3 | 209 | 59.7 | 10 | 141 | 40.3 | 3.3 |
| SUBTOTALS | 85 | 5,253 | 70.1 | 207 | 2,237 | 29.9 | 2.4 |
| TOTALS | 165 | 10,813 | 68.3 | 478 | 5,010 | 31.7 | 2.9 |

^a Figure does not include one *engenho* that was *fogo morto* ("inoperative").

^b In the parish of Oliveira, occupations were not recorded. There were probably twenty-one *lavradores de cana* who owned 133 slaves (or about 35.8 per cent of the slaves employed in sugar agriculture), but the table above has been calculated without these *lavradores* and their slaves.

distribution of slaveownership among the free population revealed in the Bahian lists provokes a number of questions. The first problem is one of "typicality." The Recôncavo was not Bahia, and Bahia was not Brazil. It is fair to ask what the slaveholding structure revealed in the 1816–17 lists represents in terms of the colony as a whole. As a major plantation zone, Bahia should present the upper levels of concentration of wealth and median and mean size of slaveholding in comparison to those of other Brazilian regions characterized by subsistence agriculture, ranching, cotton, or activities that were less labor intensive than sugar. Information to support this hypothesis is difficult to obtain, but there is enough evidence, both direct and circumstantial, to suggest its validity.

The only quantitative data available for comparison within Brazil comes from the mining zone of Minas Gerais. On the basis of local tax records and censuses for five districts for the period from 1718 to 1804, Francisco Vidal Luna has demonstrated that the size of an average slaveholding was small and that ownership of slave property was widely diffused in the population. In Minas Gerais, slaveowners with 1 to 4 slaves predominated, few held more than 40, and only one owner held more than 100 captives. During the eighteenth century, the mean size of a slaveholding varied between 3.7 and 6.5 slaves (with the Gini coefficient between 0.40 and 0.57), depending on time and place. Holdings in the mining zones were, therefore, somewhat smaller and less concentrated than those in the Recôncavo. Minas Gerais was a slaveowning economy of "small property."³⁴ The hypothesis, then, that Bahia should represent the upper levels of size and concentration of property in slaves is supported by Vidal Luna's conclusions, and, although similar quantitative studies of other regions are lacking, other, nonquantitative kinds of evidence also confirm this hypothesis.

Slavery in Brazil was broadly distributed among the free population, providing the economic basis of the society as a whole and an extremely common and accessible form of investment. Travelers often commented on the widespread and profligate use of slave labor and spoke against its deleterious moral effects.³⁵ The Englishman, Reverend Robert Walsh, for example, described seeing a black freedwoman in Rio de Janeiro with her slave:

Her young slave was her only property, and she made a good livelihood by hiring her out as a beast of burthen, to whoever wanted her, and for whatever purpose. Many persons black and white, about Rio live in the same manner. They possess a single slave, whom they send out in the morning, and exact at night a *patac*. They themselves do nothing, lying indolently about and living on this incor.³⁶

Other observers noted the use of slaves in gangs to do the labor that a single horse or pulley could have performed more quickly and efficiently. The acquisition of slaves by people of humble means and the wasteful use of slaves indicate that this

³⁴ Vidal Luna, *Minas Gerais: Escravos e senhores—Análise de estrutura populacional e econômica de alguns centros mineratórios* (Ph.D. dissertation, University of São Paulo, 1980), 157.

³⁵ This sort of observation and argument was characteristic of many of the foreign travelers in Brazil in the nineteenth century. For a guide to this literature, see Manoel Cardozo, "Slavery in Brazil as Described by Americans, 1822–1828," *The Americas*, 17 (1961): 241–60.

³⁶ Walsh, *Notices of Brazil in 1828 and 1829*, 2 (Boston, 1831): 20.

form of labor power was cheap and relatively abundant, easy to obtain, and, most important, easy to replace. The key here seems to have been the slave trade itself; throughout the colonial period, trade in slaves operated generally at levels not merely adequate to compensate for natural losses but, in certain periods, to meet demands created by economic expansion as well. Although planters invariably complained that the price of slaves was too high, evidence suggests that ownership of a slave was a relatively accessible goal that, whether for reasons of prestige or economic gain, promised a reasonable return on the investment.

The moralistic observations of foreign travelers, some of whom had their own abolitionist causes to promote, cannot always be taken at face value concerning the distribution of slavery in Brazil. And, although lists like the Bahian rolls of 1816–17 provide excellent information on the distribution of slaves among slaveowners, they cannot resolve another and perhaps more important question: what part of the Brazilian population participated through ownership in the institution of slavery? That question is more complex and difficult than it first appears. Colonial Brazilian census materials are notoriously poor and often contradictory so that simply establishing the size of the population is no easy task. And shifting the question slightly, to calculations of the distribution of wealth, should slaves themselves be counted as part of the population “at risk”?³⁷ Slaves were human beings, not just property, and they constituted about 30 percent of the total population at the close of the colonial era. Obviously, their inclusion in estimates of wealth distribution will affect the outcome. The lack of sound demographic data complicates this question in other ways. If the acquisition of wealth is in some ways cumulative, and thus linked to age, then the older an individual, the more likely he or she is to own wealth in slaves and the greater would be the number owned. Without age-specific demographic data, analyzing the distribution of slaves within a population becomes more difficult.³⁸

Given these problems, a useful way to approach the question of the diffusion of slaveownership is to ascertain the number of households that contained slaves as a function of total households. This rough measure can suggest the pervasiveness of slavery in Brazilian society. The general impression from the Bahian lists of a broad diffusion of slaveownership is supported by scattered evidence from other regions of Brazil. About one-half of the households in the urban centers of São Paulo and Ouro Preto, for example, contained at least one slave. In São Paulo the percentage decreased between 1778 and 1836, but, even at the later date, 46 percent of the free households in the town held slaves. In Ouro Preto, capital of the old mining district

³⁷ For a discussion of how to count the slaves themselves in calculations of wealth distribution, see Robert E. Gallman, “Trends in the Size Distribution of Wealth in the Nineteenth Century: Some Speculations,” in Lee Soltow, ed., *Six Papers on the Size Distribution of Wealth and Income* (New York, 1969), 1–24. And, for an interesting approach to the problem, see Richard Lowe and Randolph Campbell, “Slave Property and the Distribution of Wealth in Texas, 1860,” *Journal of Economic History*, 63 (1976): 316–24.

³⁸ The question of wealth distribution has generated a large and much-debated literature in the history of the United States. Gloria L. Main has identified some of the major trends and problems contained in this literature; see her “Inequality in Early America: The Evidence from Probate Records of Massachusetts and Maryland,” *Journal of Interdisciplinary History*, 7 (1976–77): 559–82. Also helpful to me was Jeffrey G. Williamson and Peter Lindert, “Three Centuries of American Inequality,” in Paul Uselding, ed., *Research in Economic History*, 1 (Greenwich, Conn., 1976): 69–123.

of Minas Gerais, the figure was 41 percent of the households in 1804.³⁹ This level of diffusion in urban areas is borne out by a published census for the parish of São Pedro in the city of Salvador in 1775,⁴⁰ where 47 percent of the households in that central parish contained slaves. The evidence is scattered, to be sure, but it supports the impression given by foreign travelers that slavery was a ubiquitous institution in the cities and towns of Brazil.

It is more difficult to establish the proportion of slaveholding households in rural areas. In part, the problem is a lack of sources, but in addition the variety of economic and ecological settings in Brazil makes any attempt to generalize from one region to another risky. Early evidence of a broad diffusion of slavery is provided by a census from Moucha in the cattle-ranching and subsistence-farming captaincy of Piauí. Of the 302 households recorded in the parish in 1762, slaves were present in 209 (or 69.2 percent) of them. On the cattle ranches over 90 percent of the *fogos* ("homes," "hearths") contained at least one slave.⁴¹ Similar evidence is not available from the Recôncavo, but an unpublished census of 1788 includes some parishes just beyond the Recôncavo in which the presence or absence of slaves can be calculated. Inhambupe, a district of some sugar production as well as cattle and food-crop husbandry, had 115 free *fogos* of which 56 (48.7 percent) contained slaves; and, in Agua Fria, only 6 of the 69 *fogos* (8.7 percent) did *not* contain at least one slave. In the southern captaincy of São Paulo, a region characterized in the main by a dispersed rural peasantry, about one-quarter of the *fogos* contained slaves. The proportion declined slightly between 1798 and 1818 as the captaincy moved toward production of export crops and a concentration of wealth began to take place. Small-scale slaveholding was the rule in São Paulo; in its agricultural communities about 45 percent of the slaveowners held fewer than three slaves and two-thirds of the slaveowners held six or fewer captives.⁴²

Finally, yet another demonstration of the ubiquity of slaveholding and the pervasiveness of the institution is the now substantial evidence of slaveholding by former slaves (*libertos*). Mahommah G. Baquaqua, a West African enslaved in Brazil before reaching the United States where he eventually published his story, commented that he was almost purchased by a man of color in Rio de Janeiro. "Slaveowning," he stated, "is generated in power, and anyone having the means of buying his fellow creatures with the paltry dross can become a slaveowner no matter his color, his creed, or his country, and . . . the colored man would as soon enslave

³⁹ Elizabeth Anne Kuznesof, "Household Composition and the Economy in an Urbanizing Community: São Paulo, 1765–1836," (Ph.D. dissertation, University of California, Berkeley, 1976), 135–40; and Iraci del Nero da Costa, *Vila Rica: População, 1719–1826* (São Paulo, 1979), 164.

⁴⁰ Avelino Jesus da Costa, "População de Cidade da Baía em 1775," *Actas: V Colóquio Internacional de Estudos Luso-Brasileiros*, 1 (Coimbra, 1965): 191–205.

⁴¹ Luiz R. B. Mott, "Estrutura demográfica das fazendas de gado do Piauí-colonial: Um Caso de povoamento rural centrífugo," *Ciência e Cultura*, 30 (1978): 1196–1210.

⁴² Maria Luiza Marcilio, "Fendências e estruturas dos domicílios na Capitania de São Paulo segundo as listas nominativas de habitantes, 1765–1828," *Estudos Econômicos*, 2 (1972): 131–44. Also see Emilio Willems, "Social Differentiation in Colonial Brazil," *Comparative Studies in Society and History*, 12 (1969–70): 31–49; and Table 2, page 61, above. Fernando Henrique Cardoso has pointed out the lack of concentration of slaves in the hands of powerful agriculturalists in the early years of Rio Grande do Sul's settlement; see his *Capitalismo e escravidão* (São Paulo, 1962), 47.



Figure 2: Rural slaves in Bahia in the early nineteenth century as seen by the German traveler Johann Moritz Rugendas in his *Malerische Reise in Brasilien* (Paris, 1835).

his fellow man as the white man, had he the power.”⁴³ Although the abolitionist who recorded Baquaqua’s story may have been unaware of alternative functions that slaveowning could have had for people of color, the evidence of *liberto* slaveholding is clear. A study of 259 wills and testaments left by former slaves in nineteenth-century Bahia reveals that 203 (78.3 percent) left at least one slave among their property.⁴⁴ In Minas Gerais between 1743 and 1811 *libertos* made up between 3.3 percent and 14.6 percent of the slaveholders. In the diamond-mining district of the Serro do Frio in 1738, over 22 percent of the slaveowners were former slaves.⁴⁵ The great majority of *liberto* owners held only one or two slaves, but their very presence as slaveowners indicates that even the most disadvantaged group of free people could, and did, participate in the institution of slavery. There are, in fact, even documented cases of slaves who “owned” slaves.⁴⁶

This evidence, scattered through time and space, all points in the same direction. There was a wide distribution of slavery throughout the free population, and a large segment of the population directly participated in the system. In the cities, chances were about fifty-fifty that a free individual lived in a house with a slave. In rural areas, the range was wider, depending on the local economy, extending from about 25 percent of the households in São Paulo to over 90 percent in some northeastern parishes. Furthermore, Gini coefficients calculated on this scattered data indicate that the concentration of wealth in slaves was relatively low. In sum, the existing evidence strongly suggests that slavery as an institution and a form of property pervaded Brazilian society and often touched and drew upon the participation of a broad spectrum of the population, including people of humble circumstances.

The analysis thus far is not intended to support an argument for “economic democracy” in colonial Brazil. Far from it. Whether the levels of concentration of wealth are considered “great” or “small” depends on one’s expectations and some sort of comparative perspective. The level of concentration of slaves in the Bahian Recôncavo was a moderate Gini coefficient of 0.59, a figure quite like that found for the Southern United States and quite unlike that found for Jamaica. That figure, however, says little about the distribution of wealth among the whole population. At present, data do not exist that permit an examination of the general distribution of wealth in Bahia, but this analysis of slaveholding demonstrates that its level of concentration was lower than might be expected in an export-oriented, slave-based colonial economy.

⁴³ Baquaqua, as quoted in Samuel Moore, *Biography of Mahommah G. Baquaqua, a Native of Zoogoo, in the interior of Africa . . .* (Detroit, 1854), 48. Baquaqua was employed as a baker’s assistant and then as a sailor on coasting voyages from Rio de Janeiro to Santa Catalina and Rio Grande do Sul.

⁴⁴ Maria Ines Cortes de Oliveira, “O Liberto: O Seu Mundo e os outros” (M.A. thesis, Universidade Federal da Bahia, 1979), 90.

⁴⁵ Francisco Vidal Luna and Iraci del Nero da Costa, “A presença do elemento fôrro no conjunto de proprietários de escravos,” *Ciência e Cultura*, 32 (1980): 836–81. The authors emphasized that the majority of the *liberto* slaveowners were women, but they have not provided a sex ratio of the *liberto* population so that the significance of this distribution is unclear.

⁴⁶ For the phenomenon of slaves “owning” slaves, see my “Manumission of Slaves in Colonial Brazil: Bahia, 1684–1745,” *Hispanic American Historical Review*, 54 (1974): 603–35.

COMPARISONS ACROSS TIME, SPACE, AND CULTURES are problematic, to be sure, but they do offer a context in which a specific case can be used to examine more general patterns—in this case, the owning of slaves. The *engenho* slaveholdings in the Recôncavo, to recapitulate, were generally smaller than historians have assumed; the majority of the owners held fewer than four slaves and over one-half of the slaves lived in units of fewer than twenty—all this in a region traditionally associated with a sugar plantation economy. Just how surprising these patterns really are becomes clear when we compare them with those for the other slaveholding regions of the Americas. Slaveowning seems to have been somewhat more concentrated in the mid-nineteenth century South than in late colonial Brazil, and the relative concentration of land and slaves seems to have differed in the two regions. But, *these small differences aside, what is striking is the overall similarity of slaveholding patterns in Bahia and the United States, particularly when these patterns are then compared with Caribbean slaveholding regimes.*

In the United States scholars have made an intensive examination of slaveholding patterns between 1790 and 1860. These studies have led to a series of conclusions about the inequality of wealth before the Civil War, the structure of the Southern economy, and the role of slavery within it. Many of these studies have emphasized considerable regional variations in the patterns of slaveholding according to soil type and economic activity, an approach that underscores the diversity of the Southern economy. But, despite such diversity, the findings to date also make clear that before 1850 the institution of slavery was broadly diffused within the South and that approximately one-third of all households were associated with it by ownership of slaves. In spite of a long historiographical tradition that concentrated on the plantation and the political power of the planter class, scholars as diverse as Lewis Gray, Frank Owsley, and Gavin Wright have all noted the importance of small-scale slaveowners and free farms.⁴⁷ Gray spoke of the “large number of small slaveowners—men who are hardly to be called planters. . . . [T]heir main ambition is to produce cotton and own slaves.”⁴⁸ Wright, though not wishing to overstate the case for equality of wealth in the South, still found that on many issues there was little difference between the attitudes of the large planters and those of the small slaveowners and that “the minority holding slaves was by no means tiny—roughly half of the cotton South, better than one-fourth of all families in the slave states.”⁴⁹ On the basis of these figures, Wright has argued convincingly that the stake of Southern society on the slave regime was very broadly based.

The existence of census schedules from 1790 forward has enabled historians of the southern United States to establish a firm quantitative basis for analyzing Southern slaveholding. The main thrust of many studies has been to use slaveown-

⁴⁷ For a critical review of this literature, see Wright, “‘Economic Democracy’ and the Concentration of Wealth in the Cotton South, 1850–1860,” *Agricultural History*, 44 (1970): 63–94. Albert W. Niemi, Jr., has supported for the whole South Wright’s conclusions about the cotton South; see Niemi, “Inequality in the Distribution of Slave Wealth: The Cotton South and Other Southern Agricultural Regions,” *Journal of Economic History*, 37 (1977): 747–53.

⁴⁸ Gray, *History of Agriculture in the Southern United States to 1860*, 1 (Washington, 1932): 500.

⁴⁹ Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978), 144, 42.

ership as a measure of wealth and then to compare its concentration with patterns in the North or to trace changes over time, but their findings have revealed the structure of Southern slaveholding.⁵⁰ Attention has focused on this structure in the antebellum period, especially in the years 1850 to 1860, but the work of Lee Soltow, which uses data from 1790 and 1830, is particularly useful for comparison with the late colonial materials presented here. Soltow examined a four-region area—Maryland, North Carolina, South Carolina, and the District of Columbia—in 1790 and found that an average-sized holding comprised 8 slaves and that one-third of the families held slaves.⁵¹ The Gini coefficient of concentration was 0.60, a moderate level. All of these indices change slightly for 1830, when the average holding size rose to 9.6 slaves and the Gini index barely declined to 0.59. Soltow also calculated these statistics for all fourteen Southern states in 1830 and found only minor variations from his four-region sample. His main conclusion was a notable stability in the patterns of slaveholding at least until 1830, although within these general patterns there was considerable regional diversity.⁵²

In marked contrast to the United States were the plantation islands of the Caribbean. There, the large plantations dominated, and the ratio of whites to slaves in the population was much lower than in the United States. The history of how sugar turned the Caribbean islands black has been told in other places and need not be repeated here, but the results are significant for comparative purposes. By the end of the eighteenth century, the larger British and French islands were characterized by big agricultural units and a population overwhelmingly slave. Grenada in the 1780s, for example, was over 91 percent slave; Saint Domingue in 1779 was 86 percent slave.⁵³

Jamaica, a large island with a variety of landscapes, also fits this pattern. A careful accounting of the slave population made during the period of emancipation allows us to examine the structure of Jamaican slaveholding in 1832. On Jamaica slaves totaled 86.5 percent of the population, and over one-half of them lived on great plantations with more than 150 slaves.⁵⁴ The average size of a slaveholding was 25, and the Gini coefficient of inequality was a high 0.83. Jamaica, for all intents and purposes, resembled in its overall demographic configurations the plantation writ large: slaves on large estates predominated within the population, and very large estates dominated the economy. Slaveowning in Jamaica was on a different scale from that in the United States. On that island, 61.5 percent of all slaves lived on plantations of over 100 slaves. In the United States in 1850, only 8.6 percent of the slaves lived on units of that size; even if only the Lower South is considered, that figure rises to but 14.1 percent.⁵⁵

⁵⁰ See, for example, Main, "Inequality in Early America," 559–82; Lee Soltow, *Men and Wealth in the United States, 1850–1870* (New Haven, 1975); and Williamson and Lindert, "Three Centuries of American Inequality."

⁵¹ Soltow, "Economic Inequality in the United States in the Period from 1790 to 1860," *Journal of Economic History*, 31 (1971): 822–39.

⁵² *Ibid.*, 29–31.

⁵³ Eric Williams, *From Columbus to Castro: The History of the Caribbean, 1492–1969* (New York, 1970), 104–07, 282–85; and Higman, *Slave Population and Economy in Jamaica*, 45–61, 274–75.

⁵⁴ I have based these calculations on the table in Higman, *Slave Population and Economy in Jamaica*, 274–75.

⁵⁵ Gray, *History of Agriculture in the Southern United States*, 530.

TABLE 7
Comparative Distribution of Slaveholding in Bahia, Jamaica, and the U.S. South

| <i>Place and Time</i> | <i>Mean Number of Slaves per Owner</i> | <i>Median Number of Slaves per Holding</i> | <i>Gini Coefficient of Inequality</i> | <i>Mean Proportion of Families Holding Slaves</i> | <i>Percentage of Slaves in the Total Population</i> | <i>Percentage of Free People of Color in the Total Population</i> |
|------------------------|--|--|---|---|---|---|
| BAHIA, 1816-17 | 7.2 | 29.0 | .590 | .25-.50 | 30.8 ^a | 40.0 (?) |
| JAMAICA, 1832 | 25.0 | — | .825 | — | 86.5 | 3.0 (1800) |
| U.S. SOUTH | | | | | | |
| Four Region Area, 1790 | 8.0 | — | .602 | .34 | 33.5 | — |
| Four Region Area, 1830 | 9.6 | — | .599 | .33 | — | 3.4 (1820) |
| Entire South, 1830 | 8.7 | 20.6 ^b | .597 | .36 | 33.5 | — |

^a Data for Bahia in 1819 taken from Joaquim Norberto de Sousa e Silva (1870), as presented in Maria Luiza Marcilio, "Crescimento histórico de população brasileira até 1872," *Crescimento populacional*, Centro Brasileiro de Análise e Planejamento, no. 16 (São Paulo, 1974), 14.

^b This is Lewis Gray's estimate for 1850; see his *History of Agriculture in the Southern United States to 1860*, 1 (Washington, 1932): 500.

SOURCES: For the South, Lee Soltow, "Economic Inequality in the United States in the Period from 1790 to 1860," *Journal of Economic History*, 31 (1971): 825-28; and Gray, *History of Agriculture*, 482, 529-40. For Jamaica, B. W. Higman, *Slave Population and Economy in Jamaica, 1807-1834* (Cambridge, 1976), 144-45, 374-75; and Douglas Hall, "Jamaica," in David W. Cohen and Jack P. Greene, eds., *Neither Slave nor Free: The Freedman of African Descent in the Slave Societies of the New World* (Baltimore, 1972), 194. For Bahia, see Thomas Merrick and Douglas H. Graham, *Population and Economic Development in Brazil, 1800 to the Present* (Baltimore, 1979), 66.

When the analytical data of the Bahian slaveholder lists are compared with the same sort of data for the southern United States and Jamaica, it becomes clear that the structure of slaveholding in colonial Bahia looks much more similar to that in the antebellum South than it does to that in pre-emancipation Jamaica. The proportion of slaves in the total population, the average size of holding, the coefficient of concentration of ownership, and the percentage of slaves living on large plantations in the South and the Recôncavo are far more similar to each other than either is to Jamaica (see Table 7). To be sure, Bahia as a sugar plantation zone had a higher median figure of ownership at 29.0 than the U.S. South did in 1850 at 20.6; but the lower South provides a better comparison in terms of its agricultural structure, and there the median was a remarkably similar 30.9 slaves.⁵⁶ In only one regard did Bahia probably stand far apart from both the United States and Jamaica and, in fact, from virtually all other slave-based societies of the Americas: the percentage of the Bahian population composed of free people of color was almost certainly 40 percent or higher.⁵⁷ In the Upper South, people of color were never more than 6 percent of the total free population prior to 1850, and, in the Lower South, the percentage was about one-half that figure. Of course, in the United States, free people of color had the alternative of moving to the North, but in 1820 only 3.4 percent of the total population of the United States was composed of free people of color. By 1850, that percentage had fallen to 1.8. On Jamaica, with its very small white population, free people of color in 1800 were only 3 percent of the island's people, but they composed one-third of Jamaica's free inhabitants.⁵⁸ Similar proportions were typical of the other British islands. Only on the Spanish Caribbean islands of Cuba and Puerto Rico did free people of color constitute proportions of the total population even approaching that in Brazil.

The sugar economy itself imposed certain patterns and structures on slavery and slaveholding, so that it is useful to separate Louisiana from the United States as a whole and from the South as a region in order to observe patterns in that sugar-growing area. Unfortunately, data for the early nineteenth century is not readily available, but data for 1850—thirty years later than data from the Bahian lists—is. Not surprisingly, Louisiana's structure of slaveholding was in some ways closer to that on the Caribbean sugar islands. Over 20 percent of the slaves in the state lived in units of over one hundred, compared to 8.5 percent for the South as a whole. Although large units were more characteristic of Louisiana than of any other state, still more than one-third of Louisiana's slaves resided in units of under twenty, and over one-half of the slaveowners held fewer than five slaves.⁵⁹ There are remark-

⁵⁶ *Ibid.*, 530–39.

⁵⁷ There is no adequate census of Bahia for the period in question that provides figures for the relative proportions of the free and slave population by color. Data do exist, however, for Minas Gerais in 1821, where the free people of color constituted 40 percent of the population. Travelers' observations and comments lead me to believe that the proportion in Bahia was certainly no smaller (and probably larger) than that in Minas Gerais. Especially see Herbert S. Klein, "Nineteenth-Century Brazil," in David W. Cohen and Jack P. Greene, eds., *Neither Slave nor Free* (Baltimore, 1972), 309–40.

⁵⁸ *Ibid.*, 335–40.

⁵⁹ Also see Mark Schmitz, *Economic Analysis of Antebellum Sugar Plantations in Louisiana* (New York, 1974), 127–28; and J. D. B. DeBow, *Statistical View of the United States* [Seventh Census] (Washington, 1854), 95.

able similarities between the percentile distribution of slaveholding units in the Bahian sugar parishes and Louisiana. The major difference lies in the larger percentage of slaves held in Bahia in groups of fifty to ninety-nine, reflecting the smaller size of the plantation slave force because of the *engenho's* dependence on the slaves held by the *lavradores de cana*. Without a class of cane farmers, Louisiana plantations were larger, and over 20 percent of the slaves lived in units greater than one hundred (see Table 8).

When the distribution of slaveholding according to the size of the unit for the three regions is examined, the similarity between Bahia and the United States and the dissimilarity of the two to Jamaica are plain (see Table 8). In both the U.S. South and the Bahian Recôncavo, less than 10 percent of the slaves lived in units of more than one hundred compared to over 60 percent on Jamaica. Emphasis is probably more appropriately placed on the similarity of the United States and Bahia at the bottom rather than the top of the scale of holdings—that is, in the number of slaves and owners associated with units of fewer than five slaves. In Bahia, this category comprised 64 percent of the masters and 15.4 percent of the slaves. Presumably, the figures for Brazil as a whole would be as high or higher, given its economic diversity. For the United States, it is possible to provide comparable estimates for 1790, 1830, and 1850. The trend across these sixty years was a decline in the ratio of small slaveholders to total slaveowners, but the proportion remained roughly half, decreasing from 54.9 percent in 1790 to 50.2 percent in 1850. These small owners controlled about 15 percent of the slave force in 1790, 13 percent in 1830, and 10 percent in 1850. On Jamaica, holders of fewer than five slaves were 54 percent of the owners, but these owners controlled only 4.4 percent of the island's slaves. And the ratio of those with only one slave to all slaveowners was one-half that of the United States or Bahia.

IN SEARCHING FOR THOSE ELEMENTS that explain the variations within American slave regimes, we can no longer posit the differences between Brazil and the U.S. South based on the size of slaveholding units or their distribution among the free population. The two areas were, in fact, quite similar in this regard, and both stood in sharp contrast to Jamaica and other plantation islands. The complex arrangements of slaveownership on Brazilian plantations, the existence of large numbers of small- and medium-sized slave units, and the broad distribution of slavery among the free population must all be taken into account in future discussions of the economic, social, and political impact of slavery on that colony. For example, the Bahian data demonstrate clearly that aside from the planter class there also existed a broad minority of slaveholders, hundreds of individuals or families with one or two or five slaves, whose investment in slavery was quantitatively small but whose attachment to the institution was no less real. They were the majority of the slaveholders, and they controlled a substantial portion of the slaves. This pattern of slaveholding must also be taken into account in any discussions of slave life, acculturation, familial opportunities, and rebellion. The contrasts between Brazil and the South in these matters become even more striking once the differences in

TABLE 8
Comparative Distribution of Slaves and Slaveowners
according to Size of Holding in Slaves (in Percentages)

| Size of Holding | <i>Bahia</i> | | <i>Jamaica</i> | <i>United States</i> | | |
|---|-------------------------------|------------------------------------|-------------------------|------------------------------------|------------------------------------|--|
| | <i>Recôncavo, 1816–17</i> | <i>Sugar Parishes, 1816–17</i> | <i>1832^a</i> | <i>South, 1790^b</i> | <i>South, 1830^b</i> | <i>Louisiana, 1850^b</i> |
| <i>Slaves</i> | | | | | | |
| 1–9 | 36.3 | 24.9 | 8.7 | 29.9 | 28.5 | 20.2 |
| 10–19 | 17.1 | 15.2 | 6.3 | 26.3 | 27.4 | 15.0 |
| 20–49 | 17.1 | 20.6 | 9.5 | 28.0 | 25.3 | 23.4 |
| 50–99 | 20.2 | 27.7 | 14.0 | 9.3 | 10.3 | 20.5 |
| 100–199 | 8.7 | 10.5 | 25.6 | 4.5 | 6.2 | 15.5 |
| 200 or more | 0.7 | 1.1 | 35.9 | 1.9 | 2.2 | 5.4 |
| <i>Slaveowners</i> | | | | | | |
| 1–9 | 83.6 | 76.5 | 69.1 | 79.3 | 73.4 | 73.5 |
| 10–19 | 9.5 | 11.5 | 11.2 | 13.5 | 17.1 | 12.7 |
| 20–49 | 4.3 | 6.9 | 7.5 | 6.4 | 7.7 | 8.5 |
| 50–99 | 2.1 | 3.7 | 4.6 | 1.3 | 1.4 | 3.5 |
| 100–199 | 0.5 | 1.4 | 4.4 | 0.4 | 0.4 | 1.3 |
| 200 or more | 0.0 ^c | 0.0 ^c | 3.2 | 0.0 ^c | 0.0 ^c | 0.5 |
| <i>Ratio of Owners with 1 Slave to Total Owners</i> | | | | | | |
| | .23 | .20 | .11 | .24 | .20 | .23 |

^a Calculations for Jamaica are based on Higman, *Slave Population and Economy in Jamaica*, 724–75. Higman's size categories are 1–5, 6–10, 11–20, 21–30, 31–40, etc. Such divisions inflate the percentages in the lower size units. The ratio of owners with 1 slave to total owners has been calculated by dividing the 1–5 unit by 5; this surely overestimates the ratio, since the number of owners with 2 slaves exceeds the number with 1 slave in most distributions.

^b Calculations for the South in 1790 and 1830 are based on Soltow, "Economic Inequality in the United States," 825, using a mid-point in each size category to estimate the number of slaves from each class size. The percentages for Louisiana are calculated in the same manner from J. D. B. De Bow, *Statistical View of the United States* [Seventh Census] (Washington, 1854), 95.

^c In these cases, the percentage was less than 0.1.

size and distribution are shown to be minimal, and certain similarities between these two slaveowning societies now become more understandable.

In *The World the Slaveholders Made*, Eugene D. Genovese pointed out what seemed to him to be an anomaly: slaveholders in Brazil and the southern United States closely resembled each other.⁶⁰ He argued that, despite the bourgeois origins of

⁶⁰ Genovese, *The World the Slaveholders Made* (New York, 1969), 96. Also see Genovese's vigorous and informative defense of his position in "A Reply to Criticism," *Radical History Review*, 19 (1977): 94–110.

Southern slaveholders and the seigneurial traditions of Brazilian slaveholders, both “most closely approximate the standards of paternalism we associate with the patriarchal plantation.” Genovese ascribed this similarity to the plantation regime and the contact between white and black on it, and in this he is probably correct. But what neither he nor anyone else has suspected is that the structure of slaveholding in the South and in Brazil were so similar. Genovese emphasized the importance of a resident planter class in the formation of these patriarchal societies, but what may be even more important is that the majority of those planters were resident on estates on which fewer than one hundred slaves lived, places large enough to develop all of the basic structures of everyday life but of a size that allowed the owner to know his bondsmen well enough to intervene in their daily existence in a direct and personal way. In contrast, the Jamaican plantations were usually much larger, and over one-half of the slaves of the island lived on these great estates. With three to five hundred slaves, how much difference did it make if the plantation was run by the owner or by an administrator? Neither could know all of the slaves in any but the most general way.

It would seem, in fact, that the problem in Bahia and in the United States was not that patriarchalism faced the constraints of slaveholding units that were too large but quite the opposite: the majority of slaveholders and the majority of slaves lived in residences and had working conditions in which the ideals of patriarchalism were difficult to maintain. The great aristocratic planter families set the social tenor of the slave societies, but, given the prevalence of small-scale slaveholding, it seems appropriate to seek the reasons why the prevailing attitudes of this class became so generalized. The relations of production were typified by the large plantations, but in reality neither the majority of slaveowners nor the majority of slaves interacted within that context.

Dwight D. Eisenhower and the Corporate Commonwealth

ROBERT GRIFFITH

WE KNOW MORE ABOUT DWIGHT DAVID EISENHOWER than ever before. The papers of his presidency, now open to scholars, constitute a documentary record of extraordinary richness. Scarcely a month goes by, moreover, without the appearance of yet another new book or article on his life and times. But, although we know much more about him, we do not necessarily also understand what he was about or what the significance of his presidency was. Too much of the literature seems limited by the debates of the past: Was Eisenhower an active or passive president? Was he a skillful politician or a bumbler? Was he dominant or subordinate in his relations with such powerful figures as John Foster Dulles? The answers to these questions have resulted in a useful but nevertheless limited sort of enlightenment.¹ It is possible to proceed somewhat beyond these limits, however, by focusing on what might be loosely called the political economy of the Eisenhower years: on Eisenhower's thinking about the relationship of government and the economy, on

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¹ A note on the historiography of the Eisenhower era. Most early interpretations of Eisenhower are dismissive. He had ended the Korean War and made the Republican party safe for internationalism and the New Deal, but he had provided little leadership in meeting the difficult new challenges of the mid-century, had reigned rather than ruled, was a somewhat naive and apolitical figure who, as Walter Lippmann put it, was never willing "to break the eggs that are needed for the omelette." Especially see Clinton Rossiter, *The American Presidency* (2d edn., New York, 1960). Historians seemed to agree. In a poll of seventy-five historians by Arthur M. Schlesinger in 1962, they rated Eisenhower twenty-second among American presidents, between Andrew Johnson and Chester A. Arthur; Schlesinger, "Our Presidents: A Rating by 75 Historians," *New York Times Magazine*, July 29, 1962, pp. 12, 40–41. Not until the late 1960s and early 1970s, against a backdrop of war and civil disorder, did historians and other intellectuals begin to revise this conventional portrait. See, for example, articles by Murray Kempton, Gary Wills, and Richard Rhodes, Arthur Larson's memoir, *The President Nobody Knew* (New York, 1968), and full-length studies by Herbert S. Parmet, Peter Lyon, and Charles C. Alexander. These works, despite important differences among them, portray Eisenhower as a far more complex, intelligent, and skillful chief executive. The opening in recent years of important new archival collections at the Eisenhower Library has resulted in an outpouring of books and articles extending and qualifying this reinterpretation. Especially see the work of Stephen E. Ambrose, Blanche Wiesen Cook, Robert A. Divine, Fred I. Greenstein, Richard H. Immerman, Burton I. Kaufman, Douglas Kinnard, Gary W. Reichard, and Elmo Richardson. The scholarship of these historians has been collectively labeled "Eisenhower revisionism," an unfortunately imprecise term that invites confusion with Cold War revisionism and obscures often profound differences among those so categorized.

the connections between this thought and the powerful constituencies that rallied to his support, and on the ways in which this thought shaped the politics and policies of his administration.

Eisenhower was not, of course, a profound or original thinker. He did, however, take ideas seriously, especially in the area of political economy; and, although he typically expressed these ideas in platitudes, he did create, beyond the banality of his language, a fairly coherent vision of how society ought to operate. For Eisenhower, this body of thought, which I have labeled "the corporate commonwealth," represented an attempt to resolve what he saw as the contradictions of modern capitalism and to create a harmonious corporate society without class conflict, unbridled acquisitiveness, and contentious party politics. This thought, this vision, this effort provides a unifying theme for his presidency and supplies us with at least one way in which to understand his significance.

Eisenhower was a product of the organizational revolution that had transformed American life in the twentieth century, a member of the new managerial class that led the nation's great public and private bureaucracies. Although he was not an intellectual, his thought nevertheless reflects the history, interests, and dilemmas of this new class. At West Point, he was at best an indifferent student, graduating well down in his class. His early years of military service seem to have been more crowded with coaching football and card playing than with any intellectual endeavor. His tour of duty in Panama under General Fox Connor, which he later described as "a sort of graduate school in military affairs and the humanities," was doubtless influential in his intellectual development, as was his subsequent attendance at the army's Command and General Staff School at Fort Leavenworth and the War College in Washington. So, too, was his political education, which began in the late 1920s and early 1930s, when he was attached to the office of the assistant secretary of war and charged with drafting plans for industrial mobilization in time of war, and which continued through nearly a decade of sometimes stormy service with General Douglas A. MacArthur. It was from the military itself, however, that Eisenhower absorbed the principal elements of his education: a respect for the efficiencies of organization, a contempt for politics and politicians, a distrust of popular democracy and of the masses whose "class fears and prejudices are easily aroused," and, finally, a strong commitment to duty and to the ideal of disinterested public service.²

These patterns of thought, widely shared among professional military men during the interwar years, were reinforced and deepened by the experience of command during the Second World War and by his service as army chief of staff immediately thereafter. He was especially alarmed by the strikes, inflation, and bitter partisan politics that characterized the postwar years. Nevertheless, as a

These historians nevertheless share a willingness to treat Eisenhower seriously and to see in the Eisenhower presidency an important subject for historical investigation. This essay becomes possible, at least in part, as a result of their efforts.

² Eisenhower, *At Ease: Stories I Tell to Friends* (Garden City, N.Y., 1967), 3–26, 185–87, 196–232, and "War Policies," *Infantry Journal*, 38 (1931): 489–93; and Peter Lyon, *Eisenhower: Portrait of the Hero* (Boston, 1974), 56–80. On military thought during the interwar years, especially see Morris Janowitz, *The Professional Soldier: A Social and Political Portrait* (New York, 1960), 233–56.

professional soldier, he refrained from political utterances. He did not register a party affiliation nor did he vote in presidential elections. He later confessed that, if he had voted, he would have voted Republican in 1932, 1936, and 1940 but Democratic in 1944, because of the war.³ Not until 1948, when he left the army to assume the presidency of Columbia University, did he begin to express his views in letters and speeches. Although he still had neither the time nor the inclination for wide reading or systematic reflection, the pattern of thought that emerged during these years nevertheless suggests a fairly coherent social philosophy.

AT THE HEART OF EISENHOWER'S THINKING was a struggle to reconcile and resolve the most fundamental conflicts of modern society. Industrialization, mass production and distribution, and the growth of urban populations had, he believed, all combined to create a complex, interdependent social system—a system that possessed the potential for the production of great wealth and material abundance but that was also precariously vulnerable to destruction through the selfish antagonisms of class conflict. In the nineteenth century, as he traced America's recent past, the power of "concentrated wealth" had become "a menace to the self-respect, opportunities, and livelihood of great groups of ordinary citizens" and had "compelled drastic action for the preservation of the laborer's dignity—for the welfare of himself and his family." Although the legislative reforms of the Progressive and New Deal eras had, in his view, largely ameliorated such dangers, the threat of class conflict remained. The single most important source of "all our problems," he wrote a prominent business leader in early 1952, was the disunity born of "the great chasms separating economic groupings." Not only did capital, labor, agriculture, and other interests contend among themselves, but each also sought to bend public policy to its own selfish ends: "such divisions, even though economic in origin, inevitably become so clearly reflected in political organization and doctrine that they damage both our political and economic structures, thus enlarging and perpetuating initial effects." In his diary, he posed the question in terms of Lenin's analysis of the contradictions within capitalism and of the conflict between capital and labor, between antagonistic capitalistic states, and between

³ Interview with Merriman Smith, November 23, 1954, Dwight D. Eisenhower Library, Abilene, Kans. [hereafter, DDEL], Dwight D. Eisenhower Papers as President of the United States (Ann Whitman File) [hereafter EPP], DDE Diaries series, box 3. The Ann Whitman File, a file maintained by Eisenhower's private secretary, contains some 265,000 documents that received the president's closest attention. It includes diary entries by the president and occasionally by Ann Whitman, correspondence between Eisenhower and members of his administration, close friends, and advisers, minutes of his meetings with the cabinet, legislative leaders, and national security advisers, notes and memoranda on other meetings in the Oval Office (some of which were recorded and later transcribed by Whitman), summaries of telephone conversations (which Whitman frequently monitored), and records of the president's pre-press conference briefings. Although most of the materials in this file cover the years 1953–61, there are scattered documents dated earlier and later. The Whitman file, of course, is augmented by the far larger collection of materials housed in other files, including Eisenhower's Pre-Presidential Papers [hereafter, EP], the White House Central Files, which total some six million pages, and the private papers of members of his administration, friends, and advisers. Eisenhower's correspondence during the war years, the occupation, and his tenure as army chief of staff appear in *The Papers of Dwight David Eisenhower* [hereafter, *Eisenhower Papers*], 9 vols. (Baltimore, 1970–). Numerous Eisenhower diary entries, drawn from both the Whitman file and other collections, have also recently been published; see Robert H. Ferrell, ed., *The Eisenhower Diaries* (New York, 1981).

capitalist and underdeveloped nations. Although he did not accept the inevitability of such conflicts, he did recognize that “the principal contradiction in the whole system comes about because of the inability of men to forego immediate gain for a long time good,” and he worried that “we do not yet have a sufficient number of people who are ready to make the immediate sacrifice in favor of a long-term investment.”⁴

Modern organization proved an especially difficult dilemma for Eisenhower. He extolled the new forms of corporate organization, the purpose of which, as he saw it, was “to produce orderliness, which means restriction upon irresponsible human action,” but he feared that organization also posed grave dangers for traditional economic and political liberties. If organization was necessary for the orderly conduct of human affairs, it was nevertheless “difficult to define the exact line of demarcation between rules of conduct on the one hand, and unjustifiable seizure of power on the other.” Even more threatening was the prospect that organized interests—“pressure groups,” he usually called them—would impose their narrow ends upon the state or that the state itself would become little more than a battleground for class conflict. As he told a Columbia University audience in 1948, “danger arises from too great a concentration of power in the hands of any individual or group: The power of concentrated finance, the power of selfish pressure groups, the power of any class organized in opposition to the whole—any one of these, when allowed to dominate is fully capable of destroying individual freedom.”⁵

For Eisenhower, as for other postwar conservatives, the dangers of such politicized conflict were all too readily apparent. Indeed, the Democratic party of the New and Fair Deals, built through appeals to selfish class interest, seemed to embody all of the most threatening tendencies of American democracy. In his inaugural address as president of Columbia in 1948, Eisenhower decried “demagogic appeals to class selfishness, greed, and hate” and warned against what he called “a regimented statism.” Six months later, in a commencement address, he attacked “pressure groups” and politicians who appealed “to all that was selfish in human-kind.” Speaking to the American Bar Association in September 1949—an address to which he returned in later years as the touchstone of his political philosophy—he denounced Marxian concepts of class conflict and called for the defense of freedom from “the unbearable selfishness of vested interest” and from “the blindness of those who, protesting devotion to the public welfare, falsely declare that only government can bring us happiness, security and opportunity.”⁶

Eisenhower nevertheless believed that the clash of classes was neither necessary

⁴ Eisenhower to William E. Robinson, February 12, 1952, DDEL, Robinson Papers, box 2; Speech to the American Bar Association, September 5, 1949, DDEL, EPP, Speech series, box 3; and Eisenhower to George A. Sloan, March 1, 1952, in Robert L. Branyon and Lawrence H. Larson, eds., *The Eisenhower Administration: A Documentary History*, 2 vols. (New York, 1971), 1: 23–26. For Eisenhower’s discussion of the contradictions of capitalism, see *Eisenhower Diaries* (July 2, 1953), 242–45.

⁵ Eisenhower to Robinson, February 12, 1952; and Inaugural Address, Columbia University, October 12, 1948, DDEL, EPP, Speech series, box 1.

⁶ Inaugural Address, October 12, 1948; Commencement Address, Columbia University, June 1, 1949, DDEL, EPP, Speech series, box 1; Speech to the American Bar Association, September 5, 1949; and Eisenhower to George Whitney, September 4, 1951, DDEL, EP, box 115.

nor inexorable. Indeed, the great lesson of the twentieth century was, for him at least, the interdependence of class interests, not their irreconcilability. "In our tightly knit economy, all professions and callings—no matter how widely separated they may be in purpose and technique—all have points of contact and areas of common interest," he declared in 1947. "Banker or housewife, farmer, carpenter, soldier—no one of us can live and act without effect on all the others." In early 1952 he suggested to businessman George Sloan "that agriculture, labor, management and capital frequently speak of themselves as if each were a separate and self-sufficient enterprise or community. Yet the simple fact is that each is helpless without the others; only as an effective member of an integrated team can any one of them prosper." Maximum production was possible "only when management, labor and capital work in harmony . . . ; no prosperity for one economic group is permanently possible except as all groups prosper."⁷ Drawing heavily, though perhaps unconsciously, on the social thought of Herbert Hoover, Eisenhower repeatedly called for voluntary cooperation among America's diverse economic interests. Competition and self-interest, he insisted, must be "accompanied by a readiness to cooperate wholeheartedly for the performance of community and national functions." Indeed, the secret of American success in World War II, he declared on another occasion, was that "Americans welded into a cooperative unit the enterprise, initiative, spirit and will of many million free men and women." As William E. Robinson, a close friend, noted in 1948, Eisenhower believed not in "rugged individualism in the old-fashioned Republican sense of the word" but in "freedom and independence for the individual with its collateral responsibility for cooperation."⁸ At Columbia, one of his proudest achievements was the creation of the American Assembly, in which he hoped the leaders of business, labor, government, and the professions would meet to study and plan cooperatively for the future. "We must find a way," he told the American Academy of Political Science in 1950, "to bring big business, labor, professions and government officials together with . . . experts and . . . study and work out these problems in the calmness of a nonpartisan . . . atmosphere."⁹

From his fears of class conflict and from his vision of a mutually cooperative, voluntarist society came Eisenhower's commitment to what he called "the middle way," a phrase that dominated almost all of his thinking after 1948. The "middle way" was not just a political platitude but rather signified his struggle to resolve the fundamental tensions of the modern state. The term defined not only a political position—between capital and labor, between entrepreneurial liberalism and socialism, between the Republican Right and the Democratic Left—but also a series of programmatic commitments and a style of leadership. Initially, the "middle way"

⁷ Eisenhower, "Citizenship Responsibility in Relation to National Security," May 27, 1947, Harry S. Truman Library, Independence, Mo. [hereafter, HSTL], Paul G. Hoffman Papers, box 26; and Eisenhower to Sloan, March 1, 1952.

⁸ Inaugural Address, October 12, 1948; Speech to the American Bar Association, September 5, 1949; Commencement Address, June 1, 1949; Robinson to Helen Rogers Reid, June 21, 1948, DDEL, Robinson Papers, box 9.

⁹ Speech to the American Academy of Political Science, April 26, 1950, *Proceedings of the American Academy of Political Science*, 24 (1950): 144–47; and Eisenhower to Adolphus Andrews, Jr., September 29, 1950, DDEL, EP, box 15; and Eisenhower to Edward J. Bermingham, September 29, 1950, *ibid.*, box 4.

entailed arresting the momentum of New Deal liberalism and ensuring, as he wrote a prominent business supporter, that “our economy . . . remain, to the greatest possible extent, in private hands.” In his diary, and in conversations and correspondence with friends, he worried about the dangerous “drift toward statism,” a trend that, he declared, “must be halted in its tracks.”¹⁰ Yet he was also cautious enough and realistic enough to realize that this could not be done abruptly. He agreed with former president Hoover, who warned him in early 1953 that it would be impossible to accomplish a dramatic reversal and that the best that the new administration could hope to achieve would be a gradual “flattening of the curve of this particular trend.” He believed, moreover, that at least some forms of state action were not only expedient but necessary. Government must, he argued, “prevent or correct abuses springing from the unregulated practice of a private economy” and must provide laws “necessary to an orderly and a measurably free life.” The complexities of modern economic life “require the application to all of us of commonly agreed-upon rules and regulations in order that the accidents of mass production will not defeat or destroy the right of the individual to political and economic freedom.”¹¹ More importantly, he believed that government should actively promote social harmony and encourage those mutually beneficial, voluntary, and cooperative activities that lay at the center of his vision of the good society; the essence of citizenship entailed “blend[ing], without coercion, the individual good and the common good.” Above all, he told the American Bar Association, “we need more economic understanding and working arrangements that will bind labor and management . . . into a far tighter voluntary cooperative unit than we now have.” The task of leadership, he wrote, was to bring “diversities together in a common purpose.”¹²

The greatest obstacle to this corporatist commonwealth, in Eisenhower’s view, was politics, a word he almost always used pejoratively to signify the selfish actions of special interests and classes. “Pressure groups,” he warned a Columbia audience, “often pretend to a moral purpose that examination proves to be false. The vote-seeker rarely hesitates to appeal to all that is selfish in humankind.” “When politicians begin to talk about *issues*,” he wrote a friend, “they are often talking about those things on which they feel it expedient to make extravagant promises to various pressure groups.” He distrusted popular opinion, which he believed was both uninformed and short-sighted, and he complained to friends that congressmen were oversensitive “to even transitory resentments in their several districts.” The political game, he wrote his brother Edgar, was “a combination of gossip, innuendo, sly character assassination, and outright lies” in which “the demagogue tries to develop a saleable list of items to hold before the public.” Disturbed by the bitter rhetoric of the 1948 election, with its fiery appeals to class interest, he

¹⁰ *Eisenhower Diaries* (January 14, 1949), 153–54; Eisenhower to Everett E. Hazlett, Jr., February 24, 1950, and November 14, 1951, DDEL, EPP, Name series, box 17; William E. Robinson to Bruce Barton, December 13, 1949, DDEL, Robinson Papers, box 1; and Eisenhower to Sloan, March 1, 1952.

¹¹ *Eisenhower Diaries* (June 22, 1959), 364–65; Speech to the American Bar Association, September 5, 1949; Eisenhower to Robinson, February 12, 1952; and Eisenhower to Sloan, March 1, 1952.

¹² Inaugural Address October 12, 1948; Speech to the American Bar Association, September 5, 1949; Commencement Address, June 1, 1949; and Eisenhower to William Phillips, June 5, 1953, DDEL, EPP, Name series, box 25.

confided to his diary the wish that both Democrats and Republicans would embrace the middle of the road and “choose some issues outside the nation’s economy on which to fight out elections.”¹³

Eisenhower believed that the inevitable conflicts produced by the short-sighted and self-interested actions of classes and interest groups could be resolved only through the leadership of public-spirited and professionally skilled managers such as himself, who could exercise the disinterested judgment necessary to avoid calamities such as war or depression and achieve long-range goals such as peace and high productivity. The task of such leadership was to quell the passion of the masses, to encourage self-discipline on the part of business, labor, and agriculture, and to promote the pursuit of long-term, enlightened self-interest rather than immediate gain. “To induce people to do more,” he wrote his close friend and former aide General Alfred M. Gruenther in May 1953, “leadership has the chore of informing people and attempting to inspire them to real sacrifice.” Both at home and abroad people had to be prepared to endure hardship and discipline. The real question, as he wrote to a prominent Wall Street banker, was “whether national leaders here and abroad have the courage and strength to stand up and tell the truth and to keep repeating the truth regardless of vilification and abuse, until people at large will accept and act upon the clear facts.”¹⁴

Eisenhower’s commitment to social harmony, self-discipline, limited government, and a depoliticized, administrative state all dictated, in turn, an approach to leadership that stressed restraint, patience, moderation, and flexibility. “I am convinced,” he wrote his friend Everett (“Swede”) Hazlett in 1952, “that leadership in the political as well as in other spheres consists largely in making progress through compromise.” He deplored “the table-pounding, name-calling methods that columnists so much love,” not so much because he feared “a good fight” but because he thought that “such methods are normally futile.” In a letter to a friend, he praised Lincoln as “the greatest compromiser and astute master of expediency that we have known” and confessed that he, too, was “a bit on the pragmatic side by inclination.”¹⁵ His belief in the mediatory role of government and his fear of popular politics was also reflected in his intense concern with public relations, which he saw not just as a means of political or personal aggrandizement but as a technique for defusing political conflict, limiting the role of the state, engineering support for administrative decisions, and forging consensus. He told the leaders of the Advertising Council in early 1953 that “the only way to avoid centralized domination” was “through an increased readiness to cooperate in the solution of group problems. As problems become more complex, we must find new ways to achieve cooperation—new mechanisms for discovering our problems and getting them

¹³ Commencement Address, June 1, 1949; Eisenhower to Paul G. Hoffman, February 9, 1952, HSTL, Hoffman Papers, box 26; Eisenhower to Clarence Dillon, January 8, 1953, DDEL, EPP, Name series, box 7; Eisenhower to Edgar Eisenhower, January 27, 1954, DDEL, EPP, DDE Diaries series, box 3; and *Eisenhower Diaries* (July 7, 1949), 162. Also see Eisenhower to George A. Sloan, January 29, 1952, DDEL, EP, box 100; and Eisenhower to Bradford C. Chynoweth, July 20, 1954, DDEL, EPP, DDE Diaries series, box 3.

¹⁴ Eisenhower to Gruenther, May 4, 1953, DDEL, EPP, DDE Diaries series, box 3; and Eisenhower to Dillon, January 8, 1953.

¹⁵ Eisenhower to Hazlett, October 16, 1952, DDEL, EPP, Name series, box 17; Eisenhower to Phillips, June 5, 1953; and Eisenhower to Chynoweth, July 20, 1954.

over to the American people." The Advertising Council, whose conservative messages were carried annually in hundreds of thousands of so-called public service advertisements, was just such a mechanism, Eisenhower declared, "one of our great agencies for the preservation of freedom."¹⁶

Eisenhower's belief in an American commonwealth was paralleled by a Wilsonian faith in a world order through which, as he told a London Guildhall audience in 1945, "all nations can enjoy the fruitfulness of the earth." Like most other Wilsonians, he accompanied this idealistic vision with a fairly hard-headed grasp of America's postwar needs. Foreign policy, he wrote a friend in late 1951, should be based primarily on "the need for the United States to obtain certain raw materials to sustain its economy, and, when possible, to preserve profitable foreign markets for our surpluses." There is, he wrote John Foster Dulles in 1953, a "direct connection between a prosperous and happy America and the execution of an intelligent foreign policy." Like other prominent American leaders, he feared communism not only as a military menace but also as an economic threat that would close off to America "the great industrial complex of Western Europe" and the raw material-producing nations of Africa and Asia. "Where [then] would we get the materials needed for our existence," he asked a friend in 1951. Here too, he wrote in his diary in the summer of 1953, America confronted what Marxists called the "contradictions of capitalism," both the conflict among "capitalist states for the domination of the world's surface" and the conflict "between the advanced, industrialized nations of the world and the dependent masses of backward peoples." Here too, in Eisenhower's view, the conflict, though real, was necessary and inexorable *only* if nations could not abandon their immediate selfish interests for mutual cooperation. As in domestic affairs he believed that politics was principally the expression of selfish interests and that the task of leadership was "to bring men and nations to the point where they will give to the long-term promise the same value that they give to immediate and individual gains." If we could resolve the issues of world trade and cooperation on the basis of the "long-term good of all," he concluded, "we could laugh at the other so-called 'contradictions' in

¹⁶ Leo Burnett, "Cherry Blossom Time in Washington: An Informal Report on the 'Ninth White House Conference,'" March 23–24, 1953, DDEL, James M. Lambie, Jr., Records, box 1. For an extensive discussion of the Advertising Council, see Robert Griffith, "The Selling of America: The Advertising Council and American Politics, 1945–1960," paper presented at the Forty-Sixth Annual Meeting of the Organization of American Historians, held in Detroit, April 10–12, 1981.

As Supreme Commander during World War II and later as army chief of staff Eisenhower had repeatedly lectured his subordinates on the importance of good public relations. His job as president of Columbia was also, he believed, in large measure one of public relations. He was himself, by virtually every account, a master of the art, including the skillful and highly self-conscious management of his own image. When he began his campaign for the presidency, he drew about him not only prominent figures from banking and industry but also men and women from advertising, publishing, and public relations. He made more use of professional advertising and public relations in that campaign than any previous candidate in American history. "After watching Ike deal with the press, I don't think he needs a public-relations advisor," wrote Harry C. Butcher, who during World War II served as a public relations aide to Eisenhower; Butcher, *My Three Years with Eisenhower* (New York, 1946), 20. Also see Robinson to John S. D. Eisenhower, November 22, 1954, DDEL, EPP, Name series, box 29. Among Eisenhower's close political advisors were William E. Robinson (an advertising and sales executive at the New York *Herald Tribune*, who later headed his own public relations firm and still later served as president of Coca Cola), Sigurd S. Larmon (president of the large Young & Rubicam Advertising Agency, which handled, among other accounts, Citizens for Eisenhower during the primary campaign), and Paul G. Hoffman (who had risen

our system, and . . . be so secure against the Communist menace that it would gradually dry up and wither away.”¹⁷

Like most prominent internationalists, Eisenhower generally supported the foreign and military policies of the Truman administration, yet here, too, his thinking was characterized by balance, moderation, and concern for long-term consequences. Thus, in endorsing the diplomacy of containment, he nevertheless remained more pacific than most of his military and civilian contemporaries. America must not seek “to preserve order in the sense of the Roman Peace, where one nation, due to its dominant position in the world, rules all others,” he warned newspaper columnist Dorothy Thompson. To his father-in-law, he wrote, “We are traveling a long and rocky road toward a satisfactory world order but the big thing is that we never give up for an instant. No war can be anything else but a grave setback to such progress. The one thing that disturbs me is the readiness of people to discuss war as a means of advancing peace. To me this is a contradiction in terms.” Eisenhower did not share the feeling of vulnerability that pervaded so much of the military during the early postwar era. “It is a grievous error to forget for one second the might and power of this great Republic,” he cautioned Walter Bedell Smith in late 1947. He believed that both Soviet power and intent were limited and that in dealing with the Russians the United States ought to employ “patience, tolerance and a spirit of understanding.” Following the Berlin blockade and Korea, he adopted a more hardline attitude toward the Russians, but he refused even then to be stampeded by those who believed that Armageddon was just around the corner.¹⁸

He believed in military preparedness, but only in moderation. As army chief of staff he had worked on behalf of military unification and universal military training and had attempted, without success, to restrain the abrupt contraction of defense budgets that followed the end of the war. He nevertheless believed that resources were limited and that armed forces were, as he put it, “nonproductive, sterile organizations whose purposes are, at the best, largely negative.” By early 1952, following the rapid military expansion that accompanied the Korean War, he

through sales and advertising to head Studebaker, had been a founder of the Committee for Economic Development, had served as an administrator of the Marshall Plan under Truman, and was then President of the Ford Foundation). On the role of advertising and public relations in the 1952 campaign, especially see Stanley Kelley, Jr., *Professional Public Relations and Political Power* (Baltimore, 1956), chaps. 4–5; and Melvyn H. Bloom, *Public Relations and Presidential Campaigns: A Crisis in Democracy* (New York, 1973), chap. 3. These studies, if anything, understate the pervasiveness of professional advertising and public relations in the campaign, during the course of which Eisenhower employed—in one capacity or another—Young & Rubicam, Batten, Barton, Durstine and Osborn, the Kudner Agency, Ted Bates and Company, McCann Erickson, the Gallup Poll, and the Nielson rating service. His campaign, moreover, attracted the talents of many corporate advertising and public relations executives, including Charles F. Moore, Jr., of the Ford Motor Company and Abbott M. Washburn of General Mills.

¹⁷ Eisenhower, “I Am Now a Londoner,” Speech of June 12, 1945, DDEL, EPP, Speech series, box 3; Kevin McCann, ed., *Eisenhower’s Creed* (New York, 1952), 12; Eisenhower to John Foster Dulles, November 16, 1953, DDEL, EPP, DDE Diaries series, box 3; Eisenhower to Edward J. Bermingham, February 28, 1951, DDEL, Robinson Papers, box 1; and *Eisenhower Diaries* (July 2, 1953), 242–45. Also see Eisenhower to M. W. Clement, January 9, 1952, DDEL, EP, box 24.

¹⁸ Eisenhower to Thompson, June 25, 1946, in *Eisenhower Papers*, 7: 1149–50; Eisenhower to John Sheldon Doud, August 23, 1946, *ibid.*, 8: 1249–50; and Eisenhower to Smith, November 28, 1947, *ibid.*, 2084–85.

was worried over the expenditure of “unconscionable sums” for an indefinite duration. As in foreign and domestic affairs, concern for the enduring integrity of the system was the controlling issue. Every expenditure, he insisted, “must be weighed and gauged in the light of probable long-term *internal* effect.”¹⁹ Here again, however, selfish interests often threatened the collective good. The danger arose not just from the short-sighted partisanship of congressional budget-cutters but oftentimes from the service bureaucracies themselves. During World War II and again during the battles over unification and joint strategic planning, Eisenhower struggled to strike a balance among the competing claims of the services, and his letters and diaries are filled with angry denunciations of military self-interest. Here too, reconciliation of conflict and pursuit of the national interest rested with disinterested professional leadership and with the self-discipline and commitment to long-term goals of competing parties.²⁰

For Eisenhower, then, the corporate commonwealth was not just a series of vague generalities, but a broad, internally consistent, social philosophy that brought together an interpretation of America’s recent past, a vision of the good society both at home and abroad, and a style of leadership through which such an order might be obtained.

EISENHOWER’S VISION OF A CORPORATE COMMONWEALTH did not, of course, originate with him. Indeed, variations of this concept—with its emphasis on organization, cooperation, and social harmony—go back at least to the Progressive era and the National Civic Federation, to the businessmen such as Bernard M. Baruch and Gerard Swope who served on the War Industries Board during the First World War, to Herbert Hoover and his advocacy of the “associative state,” to Edward Filene, Henry S. Dennison, and other apostles of welfare capitalism in the 1920s, and to the big businessmen who during the New Deal joined the Department of Commerce’s Business Advisory Council (BAC) and helped shape the structure and operation of the National Recovery Administration. Common to all of these activities was an attempt to fashion a new corporative economy that would avoid both the destructive disorder of unregulated capitalism and the threat to business autonomy posed by socialism.²¹

¹⁹ Eisenhower to Eric Larrabee, March 25, 1947, in *Eisenhower Papers*, 8: 1631; and *Eisenhower Diaries* (January 22, 1952), 209–13. Also see Eisenhower to M. W. Clement, December 4, 1951, DDEL, EP, box 24.

²⁰ *Eisenhower Diaries* (July 24, 1947), 142; *ibid.* (January 8, 1949), 152; *ibid.* (January 27, 1949), 154–55; and Eisenhower to Everett Hazlett, April 7, 1949, and February 24, 1950, DDEL, EPP, Name series, box 1. “There are few people outside the Armed Services and the higher echelons of the State Department that are giving their full attention to American interests as a whole and refusing to color their conclusions and convictions with the interests of party politics,” he wrote Walter Bedell Smith in late 1947. “I should like to be numbered among this disinterested group.” Eisenhower to Smith, September 18, 1947, in *Eisenhower Papers*, 9: 1933–34.

²¹ Ellis W. Hawley has defined a “corporative system” as “one whose basic units consist of officially recognized, non-competitive, role-ordered occupational or functional groupings . . . , one with coordinating machinery designed to integrate these units into an interdependent whole and one where the state properly functions as coordinator, assistant, and midwife rather than director or regulator. In such a system there are deep interpenetrations between state and society, and enjoying a special status is an enlightened social elite, capable of perceiving social needs and imperatives and assisting social groups to meet them

In spite of their considerable power and prestige, "corporate liberals" remained a minority voice in national politics and even within the business community itself. By the mid-1930s, moreover, the depression and the sometime radicalism of the New Deal had disrupted their attempts to forge an alliance between business and government. After 1934 all but a handful of business leaders abandoned efforts at collaboration with the Roosevelt administration, while the president, in turn, now excoriated the "economic royalists." World War II, however, served to reinvigorate corporate liberalism. Mobilization brought thousands of executives into government, effectively dampened New Deal criticism, and created, at least temporarily, the kind of partnership between business and government that corporate liberals had long extolled. In addition, the wartime revival of corporate liberalism gave birth to important new organizations, such as the Committee for Economic Development (CED) and the Advertising Council.²²

The relationship of corporate liberals to the Roosevelt and Truman administrations, however, remained uneasy. On the one hand, they welcomed cooperation between business and government and recognized that the state could serve as a powerful positive instrument for moderating economic conflict, regulating domestic markets, promoting international trade, and sustaining economic growth. On the other, they feared that the popularity of the New Deal and the power of its progressive constituencies might lead, piecemeal, to a semisocialist state whose fiscal and regulatory policies would ultimately destroy private enterprise. As William Benton, one of the founders of the Committee for Economic Development, wrote in 1944, the leaders of government ought to "rid the economy of injurious or unnecessary regulation, as well as administration that is hostile or harmful," and pursue "constructive fiscal, monetary and other policies that provide a climate in which a private enterprise system can flourish." Businessmen, for their part, he continued, must learn to cooperate with government in the exercise of

through enlightened concert of interests." Hawley, "The Discovery and Study of a 'Corporate Liberalism,'" *Business History Review*, 52 (1978): 309–20. On the National Civic Federation, see James Weinstein, *The Corporate Ideal in the Liberal State* (Boston, 1968); for Hoover, see Hoover, *American Individualism* (New York, 1922); Joan Hoff Wilson, *Herbert Hoover: Forgotten Progressive* (Boston, 1975); and especially Ellis W. Hawley, "Herbert Hoover, the Commerce Secretariat, and the Vision of an 'Associative State,'" *Journal of American History*, 61 (1974–75): 116–40; on welfare capitalism, see Stuart D. Brandes, *American Welfare Capitalism, 1880–1940* (Chicago, 1976); and, on the Business Advisory Council, see Kim McQuaid, "Corporate Liberalism in the American Business Community, 1920–1940," *Business History Review*, 52 (1978): 342–67. Historians have used the term "corporate liberalism" to describe both the historical process through which liberal institutions have been adapted to the imperatives of large-scale organization and the ideology that has been used to promote and legitimize such a process. "Corporate liberals" have differed sharply among themselves on such important questions as the extent of state intervention, and historians have used the label to describe a fairly broad range of political behavior. Eisenhower's emphasis on voluntarism and cooperation most closely resembled the thought of Herbert Hoover. The history of corporate liberalism in America invites comparison and contrast with corporatist process and ideology outside the United States; see Matthew H. Elbow, *French Corporate Theory, 1789–1948* (New York, 1953); Ralph Bowen, *German Theories of the Corporative State* (New York, 1947); Samuel H. Beer, *British Politics in the Collectivist Age* (New York, 1965); Nigel Harris, *Competition and the Corporate Society* (London, 1972); Frederick B. Pike and Thomas Stritch, eds., *The New Corporatism: Social-Political Structures in the Iberian World* (South Bend, Ind., 1974); and especially Charles S. Maier, *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I* (Princeton, 1975).

²² Robert M. Collins, *The Business Response to Keynes, 1929–1964* (New York, 1981); and Griffith, "The Selling of America."

those powers that had become clearly necessary in a modern economy and display a “high degree of imagination, goodwill and inventiveness” in order to work out “improved rules of the game.” If business failed to plan for the postwar, warned Paul G. Hoffman, another CED founder, “the Government will be forced to step in, and collectivism will come to postwar America—by default rather than design.” “We must plan carefully and strike hard,” one of Hoffman’s business correspondents bluntly wrote. “Otherwise the new dealers will plan for all of us.”²³ The strategic problem faced by corporate liberals in the postwar era was, thus, how to obtain the benefits of state intervention, while avoiding its dangers. Their immediate, tactical problem was how to win political power in an era still dominated by the passionate and well-remembered struggles of the 1930s. Not surprisingly, they quickly discovered in Eisenhower the solution to both dilemmas. As a widely admired war hero, he was pre-eminently electable. More importantly, as they were soon to discover, his political and economic views in many ways closely approximated their own.

No single generalization covers all the men and women who rallied about Eisenhower in the late 1940s, but the great majority of them were corporate liberals for whom the goals of cooperation and consensus were paramount, who saw in the New Deal a potentially dangerous threat to corporate enterprise, and who sought to refashion the federal state to their own needs. Some were progressive manufacturers such as Hoffman of Studebaker, Thomas J. Watson of I.B.M., Philip D. Reed of General Electric, and Harry A. Bullis of General Mills. Others, such as Clifford Roberts and John Hay Whitney, represented the financial community. Still others—William E. Robinson, Douglas M. Black, Eugene Meyer, and the Ogden Reid family—were in what might loosely be called “cultural production”—journalism, publishing, advertising, and public relations. Almost all were associated with the “internationalist” wing of the Republican party and frequently held membership in such groups as the Business Advisory Council, the Committee for Economic Development, the Advertising Council, and the Council on Foreign Relations.²⁴ Most of them would have agreed with William Robinson, who wrote Eisenhower in 1951 that the managerial revolution had created in America a new form of economic democracy in which management acted as “a referee between the three major elements in our economy—the customer, the worker and the capitalist”—or with Edward J. Bermingham, who insisted that “capitalism” was an inadequate description for the American economy, which was “an overall system of individual endeavor profitable to management, labor, and ownership, with vast numbers of labor among the stockholders and thus having ownership in their business.” They shared Eisenhower’s concern over “the insidious

²³ Benton, “The Economics of a Free Society,” *Fortune*, October 1944, pp. 163–65; and Hoffman, as quoted in *The Republican*, April 1943, p. 1. Also see Hoffman, Speech to the Advertising Federation of America, June 30, 1943, HSTL, Hoffman Papers, box 104; and Lou Holland to Hoffman, March 20, 1943, HSTL, Lou Holland Papers, box 163.

²⁴ Herbert S. Parmet, *Eisenhower and the American Crusades* (New York, 1972), 33–34; Lyon, *Eisenhower: Portrait of the Hero*, 373–410; Ellis D. Slater, *The Ike I Knew* (New York, 1980), 3–27; and Clifford Roberts, *The Story of the Augusta National Golf Club* (Garden City, N.Y., 1976), 112–32.

inclination toward statism" and enthusiastically endorsed his call for "middle-of-the-road" government.²⁵

These influential business and political leaders were convinced, out of both ideology and expedience, that the election of Eisenhower was a necessity. On the one hand, they greatly feared the implications of continued Democratic rule; on the other, they distrusted the views of Republican Senator Robert A. Taft of Ohio, especially on foreign policy. More importantly, they were certain he could not win. As Eisenhower later recalled in a letter to his brother Edgar,

In 1948, '49, '50, '51 and early '52, many hundreds of people were urging me to go into politics. Scores of different reasons were advanced as to why I should do so, but in general they all boiled down to something as follows: "The country is going socialistic so rapidly that, unless Republicans can get in immediately and defeat this trend, our country is gone. Four more years of New Dealism and there will be no turning back. This is our last chance."²⁶

No one put it more bluntly than Thomas E. Dewey, who in 1949 told Eisenhower that only he could "save this country from going to Hades in the handbasket of paternalism-socialism-dictatorship." The problem, declared the twice-defeated presidential aspirant, was that, although "all middle-class citizens of education have a common belief that tendencies toward centralization and paternalism must be halted and reversed, no one who voices these views can be elected." This meant, Dewey continued, that "we must look around for someone of great popularity and who has not frittered away his political assets by taking positive stands against national planning, etc., etc. Elect such a man to the Presidency, *after which* he must lead us back to safe channels and paths."²⁷

The support of business men such as Robinson and Hoffman, publishers such as Helen Rogers Reid and Henry Luce, and politicians such as Dewey and Henry Cabot Lodge became absolutely critical to the Eisenhower campaign. These men and women not only convinced Eisenhower that he had a duty to run for the presidency but also reinforced his views on political economy, marshalled enormous financial and editorial support for his candidacy, and managed to force his nomination upon the reluctant regulars of the Republican party. Taft was more correct than not when he bitterly complained in the wake of the Republican convention that Eisenhower had been installed by "the power of the New York financial interests and a large number of businessmen subject to New York

²⁵ See, for example, Robinson to Eisenhower, March 4, 1949, September 6, 1949, and March 13, 1951, DDEL, Robinson Papers, box 1; Robinson to Reid, March 2, 1947, *ibid.*; Horace C. Flanagan to Robinson, February 18, 1948, *ibid.*; Clarence Budington Kelland to Robinson, September 1949, *ibid.*; Clifford Roberts, Memo, January 1950, *ibid.*; Edward J. Bermingham to Eisenhower, March 14, 1951, as cited in Lyon, *Eisenhower: Portrait of the Hero*, 397; Paul G. Hoffman to Eisenhower, December 5, 1951, HSTL, Hoffman Papers, box 26; and George Whitney to Eisenhower, January 16, 1952, DDEL, EP, box 115.

²⁶ Eisenhower to Edgar Eisenhower, May 2, 1956, DDEL, EPP, DDE Diaries series, box 9. Clarence Dillon wrote that Eisenhower was "the one best hope for the salvation of Western Civilization as we know it"; Dillon, as quoted in Robinson to Eisenhower, December 21, 1950, DDEL, Robinson Papers, box 11. Clifford Roberts urged Eisenhower to help arrest "the current trend of socialism and the serious threats to our liberty"; Roberts, Memo, January 1950. And, according to Paul Hoffman, only Eisenhower could "redeem the Republican Party"; Hoffman to Eisenhower, December 5, 1951. Also see *Eisenhower Diaries* (October 1949–February 1952), 165–205 *passim*.

²⁷ *Eisenhower Diaries* (July 7, 1949), 161–62 (italics in original MS). Clare Booth Luce told Eisenhower approximately the same thing; *Eisenhower Diaries* (September 27, 1949), 163.

influence" and by the nation's great newspaper, many of which "turned themselves into propaganda sheets."²⁸ The concept of a corporate commonwealth was, then, not just an exercise in political platitude but an ideology that rationalized a critically important development in American economic life and mobilized a powerful constituency behind the election of Eisenhower as president of the United States.

THE VISION OF A CORPORATE COMMONWEALTH, which was the touchstone of Eisenhower's political philosophy and which drew to his support so many progressive capitalists, also shaped the policies and politics of his administration. As president, Eisenhower sought to create a noncoercive, self-disciplined, and harmonious corporate society by limiting the New Deal state, forging cooperative relations between business and government, promoting social harmony and consensus at home, and maintaining a stable and Western-oriented international order abroad. These efforts represented a serious, in many ways even sophisticated, attempt to escape the dilemmas created by modern economic organization. They also revealed the sharp limitations of such an ideology, its class bias, and its profoundly antidemocratic character.

Eisenhower believed that the federal state posed a dangerous threat to economic liberties, and he was determined to arrest the growth of government and slowly but firmly to "bend the curve" away from public enterprise. He lifted wage-price controls, initiated new policies to prevent government competition with business, reduced the federal budget and lowered taxes on industry and capital. He signed legislation turning over oil-rich "submerged lands" to the states and strongly supported deregulation of natural gas. He withdrew federal opposition to private hydroelectric development in Idaho and California and sought to prevent further expansion of the Tennessee Valley Authority. In the critical new field of nuclear energy, he sponsored legislation that ended exclusive federal control of development.²⁹

Eisenhower was not, however, a conservative ideologue, intent on returning the

²⁸ Taft, Memorandum on the 1952 Campaign, Library of Congress, Robert A. Taft Papers, box 1349. "It is hard for me to understand the attitudes of the businessmen, bankers and editors who seem to be determined to defeat any real Republican administration," wrote a bitter Taft shortly after his defeat; Taft to Hugh Butler, August 8, 1952, State Historical Society of Nebraska, Lincoln, Hugh A. Butler Papers, box 363. Taft was only partially correct, according to William Robinson, who later recalled that "Wall Street was about 3-to-1 in his corner." Taft was, however, "right about the newspapers. But what he didn't know was that some of us had been building up 'cells' of Eisenhower interest and enthusiasm among editors and publishers in every nook and corner of the country since about 1947. It is not strange that the Senator would not have known about this, since only a handful of people was even aware of it." Robinson to Eisenhower, DDEL, EPP, November 25, 1959, Name series, box 29.

²⁹ On wage-price controls, see Eisenhower, Press Conference, February 17, 1953, in *Public Papers of the Presidents of the United States: Dwight D. Eisenhower, 1953-1961* [hereafter, *Eisenhower Public Papers*], 8 vols. (Washington, 1960-61), 1: 45-47; Cabinet Meetings, February 6 and 12, 1953, DDEL, EPP, Cabinet series, box 1; and Eisenhower to Taft, January 9, 1953, DDEL, EPP, Name series, box 32. On public competition with business, see Cabinet Paper, "Government Competition with Business," January 15, 1955, DDEL, EPP, Cabinet series, box 4; and "Report on the Bureau of the Budget . . . , 1953-1961," January 19, 1961, DDEL, EPP, Administration series, box 37. On the federal budget, see especially *Congress and the Nation, 1945-1964: A Review of Government and Politics in the Postwar Years* (Washington, 1965), 361-74, 387-92. On taxes, see Annual Budget Message, January 21, 1954, in *Eisenhower Public Papers*, 2: 90-99; Council of Economic Advisors, "The Administration Program for Economic Expansion," May 17, 1954, DDEL, EPP,

nation's economic relations to those of 1900. He clearly recognized that the state must play an active role in sustaining high productivity and employment. As he wrote his brother Milton in early 1954, "Maintenance of prosperity is one field of governmental concern that interests me mightily and one on which I have talked incessantly to associates, advisers, and assistants ever since last January. In these days I am sure that government has to be the principal coordinator and, in many cases, the actual operator for the many things that the approach of depression would demand."³⁰ Like most conservatives, however, he feared inflation more than unemployment and was willing to accept slower growth and higher joblessness in return for wage and price stability. He preferred to act cautiously and often indirectly, avoiding a highly visible or intrusive federal presence. He believed strongly, moreover, in the necessity for self-discipline on the part of both business and labor. As he declared in 1957, the national interest must take precedence over the temporary advantages that might be secured by particular groups at the expense of all the people. "Should we persistently fail to discipline ourselves," he warned, there would be inexorable pressure for government to intervene and "freedom will step by step disappear."³¹

Cabinet series, box 3; and *Congress and the Nation, 1945–1964*, 415–17. On submerged lands, see "Notes on the President's Meeting with Congressional Leaders," January 26, 1953, DDEL, EPP, Legislative series, box 1; and Sherman Adams to Joseph Dodge, March 23, 1953, *ibid.*; and, for background, see Ernest R. Bartley, *The Tidelands Oil Controversy* (Austin, Texas, 1953); and *Congress and the Nation, 1945–1964*, 1401–04. On national gas legislation, see *Eisenhower Diaries* (February 11, 1956), 314–16; Eisenhower to H. R. Cullen, February 29, 1956, DDEL, EPP, DDE Diaries series, box 7; and Eisenhower to Sid Richardson, February 26, 1956, *ibid.*; and, for a fuller discussion of the natural gas issue, see Robert Engler, *The Politics of Oil: Private Power and Democratic Directions* (Chicago, 1961), 403–21; and Gerald D. Nash, *United States Oil Policy, 1890–1964: Business and Government in Twentieth-Century America* (Pittsburgh, 1968), 209–37. On public power, especially see Department of the Interior, "Power Policy," July 21, 1953, DDEL, EPP, Cabinet series, box 2; and Cabinet Meeting, July 31, 1953, *ibid.*; and, for the best brief summary of public power issues during the Eisenhower years, see *Congress and the Nation, 1945–1964*, 832–966, *passim*. On TVA, especially see J. M. Dodge, Proposed Statement with Regard to TVA, November 23, 1953, DDEL, EPP, Administration series, box 13; and also see *Congress and the Nation, 1945–1964*, 908–31; and Aaron Wildovsky, *Dixon-Yates: A Study in Power Politics* (New Haven, 1962). On atomic energy, see Walter Adams and Horace M. Gray, *Monopoly in America: The Government as Promoter* (New York, 1955), 163; *Congress and the Nation, 1945–1964*, 935–39; and Eisenhower, Message on Atomic Energy, February 17, 1954, in *Eisenhower Public Papers*, 2: 260–69. For a general account of resource policies during the Truman and Eisenhower years, see Elmo Richardson, *Dams, Parks, and Politics: Resource Development and Preservation in the Truman-Eisenhower Era* (Lexington, Ky., 1973).

³⁰ "Means available to the government include revision of tax laws to promote consumption, extension of credit and assuring of low interest rates; vigorous liberalization of all social security measures, extension of all kinds of reinsurance plans, as well as direct loans and grants; acceleration of construction programs involving everything from multiple purpose dams, irrigation projects, military equipment and public buildings on the one hand, to increased expenditures for soil conservation, upstream water storage and public housing on the other. There are, of course, other things the Government can and would do." Eisenhower to Milton Eisenhower, January 6, 1954, DDEL, EPP, Name series, box 12. Eisenhower's attentiveness to economic issues is revealed in frequent discussions with the Cabinet and, to a lesser extent, in meetings with Congressional leaders, in memoranda and private correspondence in the DDE Diaries series, and in extensive correspondence with economic advisers such as Arthur Burns and Gabriel Hauge, much of which is located in the Eisenhower Library, EPP, Administration series, boxes 9, 10, 19. Eisenhower quite typically acted indirectly and through subordinates in seeking to influence economic policy—for example, his efforts to put pressure on the Federal Reserve Board to relax credit restrictions in 1954 and 1956; *Eisenhower Diaries* (April 8, 1954), 277–78; Eisenhower, Telephone conversations, April 21, 1956, and May 17, 1956, DDEL, EPP, DDE Diaries series, box 8; Eisenhower to Harold Stassen, May 18, 1956, *ibid.*; and Eisenhower to Lewis W. Douglas, September 30, 1956, DDEL, EPP, Administration series, box 13. On Eisenhower's fiscal policies, also see Herbert Stein, *The Fiscal Revolution in America* (Chicago, 1969), 281–371.

³¹ Eisenhower, Annual Message, January 10, 1957, in *Eisenhower Public Papers*, 5: 20. The danger, he

Eisenhower was also committed to at least maintaining the modest social welfare programs that had emerged from the New Deal. "Social gains," he told the Western Governors' Conference in 1952, were "not an issue any more" but a necessary "floor that covers the pit of disaster." He recognized what he called "the practical necessity of establishing some kind of security for individuals in a specialized and highly industrialized age" and criticized business leaders who in the past had been "far too slow to understand the implications of the continuing social revolution" as well as "far too apt to take a completely indefinite, if not essentially a selfishly cruel, attitude toward the whole question." He also had a keen sense of the expedience of such programs. As he patiently explained to Edgar Eisenhower, the most conservative of the brothers, "Should any political party attempt to abolish Social Security, unemployment insurance, and eliminate labor laws and farm programs, you would not hear of that party again in our political history."³² In 1954, at his urging, Congress expanded Social Security to as many as seven and a half million additional Americans, bringing fully five-sixths of the nation's paid work force under the law's provisions. He was less successful, however, in extending the minimum wage; despite his request that coverage be broadened to include "millions of low-paid workers now exempted," Congress declined to act. As late as 1960, minimum-wage protection applied only to a little over one-half of the nation's wage workers.³³

Eisenhower's support for social welfare programs nevertheless remained sharply limited by his reluctance to enlarge the federal budget, raise taxes, or initiate new programs, the responsibility for which, in his view, lay clearly with the states. He strongly opposed national health care, proposing instead a plan whereby the government would reinsure private insurance companies against heavy losses in order to encourage them to expand their coverage.³⁴ He was reluctant to endorse federal aid to education, justifying his support for the National Defense Act of 1958 by stressing the "national security aspect" of the program and what he called "the dominating need for scientists."³⁵ During the last years of his presidency, fearful of inflation and facing large Democratic majorities in Congress, he waged a stubborn

declared, was "always present, particularly if the Government might become profligate or private groups might ignore all possible results on our economy of unwise struggles for immediate gains."

³² Transcript, Western Governors' Conference, August 20, 1952, DDEL, EPP, Speech series, box 3; Eisenhower to Bradford C. Chynoweth, July 13, 1954, DDEL, EPP, DDE, Diaries series, box 4; Eisenhower to J. Earl Schaefer, September 30, 1954, *ibid.*; Eisenhower to Edgar Eisenhower, November 8, 1954, *ibid.*, box 5; Eisenhower to E. F. Hutton, October 7, 1953, *ibid.*, box 2; and Eisenhower to Milton Eisenhower, November 6, 1953, *ibid.*

³³ Eisenhower to Milton Eisenhower, November 6, 1953; and *Congress and the Nation, 1945-1964*, 641-44, 1249-53. Eisenhower opposed, but did not veto, the disability insurance program enacted in 1956 as well as increases in the federal share of public assistance monies in 1956 and 1958.

³⁴ Oveta Culp Hobby, Memorandum for the President, May 14, 1954, DDEL, EPP, Administration series, box 20. For the views of the insurance industry's chief lobbyist, see Eugene M. Thoré, Address of May 5, 1954, *ibid.* Even this modest proposal drew sharp opposition from the American Medical Association, which effectively blocked its passage. "How in the hell is the American Medical Association going to stop socialized medicine if they oppose such bills as this," declared an angry Eisenhower. "If they [the American people] don't get a bill like this, they will go for socialized medicine sooner or later and the Medical Association will have no one to blame but itself." James C. Hagerty Diary, July 14 and 19, 1954, DDEL, Hagerty Papers, box 1. A subsequent proposal, put forth by Hobby's successor, Marion Folsom, urged the relaxation of antitrust laws in order to enable insurance companies to pool their resources and thereby extend coverage, but this, too, made little headway; *Congress and the Nation, 1945-1964*, 1153; and James L. Sundquist, *Politics and Policy: The Eisenhower, Kennedy, and Johnson Years* (Washington, 1968), 292-93.

³⁵ On the NDEA, see Cabinet meeting, December 2, 1957, DDEL, EPP, Cabinet series, box 10; and Legislative Leadership meeting, December 4, 1957, DDEL, EPP, Legislative series, box 2. For the

and often effective battle against the further expansion of social welfare programs: "In my special seat I feel that it is not enough to know that an activity and its expenditure are merely *desirable*. We must be quite clear in the establishment of priorities so that these things can be done at a *rate* and in a sequence that will conform to the fiscal facts, as well as the clear requirements, of a nation that operates, primarily, on the *free enterprise system*." During the years of his presidency, federal transfer payments, the surest index to social welfare policy, remained unchanged as a percentage of the federal budget.³⁶

EISENHOWER COMBINED HIS ATTEMPT to limit the role of the federal state with a strong and pervasive emphasis on cooperation between business and government. He drew about him a cabinet whose members he hoped would share his own faith in partnership and corporate self-government; he appointed representatives of business and industry to important regulatory boards and commissions; and he greatly expanded the government's already elaborate network of industrial advisory committees. The influence of existing groups such as the Business Advisory Council (BAC) and the National Petroleum Council was increased, while many new advisory committees were created. At the Department of Commerce, for example, Secretary Sinclair Weeks announced, following wide consultation with industry and trade association leaders, the creation of a new Business and Services Administration, which would preside over an extensive network of advisory committees designed to help allocate materials required by defense and atomic energy programs and make recommendations on applications for accelerated tax amortization, federal loan assistance, stockpiling, and other matters. Every department, Eisenhower assured the BAC in early 1953, was engaged in organizing similar bodies on a more or less formal or informal basis.³⁷ Similarly, the famous stag dinners to which he invited the leaders of America's great corporations were not just ritual celebrations of success but meetings at which Eisenhower hoped to

importance of scientific and engineering training in the minds of the president and his advisers, see Thomas S. Nichols, Memorandum for the President, April 26, 1955, DDEL, EPP, Administration series, box 30; Bryce C. Harlow, Memorandum for the Record (of discussion between Eisenhower and David Sarnoff), April 26, 1956, *ibid.*, box 19; and Memorandum for the President (on the Report of the President's Committee on Education beyond the High School), October 23, 1957, *ibid.*, box 16. For a recent study of the NDEA, see Barbara Barksdale Clowse, *Brainpower for the Cold War: The Sputnik Crisis and the National Defense Education Act of 1958* (Westport, Conn., 1981).

³⁶ Eisenhower to I. S. Ravidin, September 19, 1959, DDEL, EPP, DDE Diaries series, box 28. Also see Charles C. Alexander, *Holding the Line: The Eisenhower Era, 1952-1961* (Bloomington, Ind., 1975), 103.

³⁷ Eisenhower's appointments were more heavily Protestant (85 percent) than were Truman's, more likely to be political independents (20 percent), more likely to be recruited from the private sector (over 70 percent), and more likely to be from elite educational institutions. Indeed, despite the educational backgrounds of Roosevelt and Kennedy, "the ivy influence was at its peak under Eisenhower." David T. Stanley *et al.*, *Men Who Govern: A Biographical Profile of Federal Political Executives* (Washington, 1967), 16-32, *passim*. On Eisenhower's appointments to regulatory boards and commissions, see Engler, *The Politics of Oil*, 323-25, 330-31, *passim*; David A. Frier, *Conflict of Interest in the Eisenhower Administration* (Ames, Iowa, 1969); and Bernard Schwartz, *The Professor and the Commission* (New York, 1959), 140-43, *passim*. For an excellent discussion of the evolution of the advisory system, see Grant McConnell, *Private Power and American Democracy* (New York, 1966), chap. 8. For Eisenhower's remarks to the BAC, see *Eisenhower Public Papers* (March 18, 1953), 1: 103. The use of industry advisory committees and unpaid consultants ("WOCs," meaning "without compensation") was extensively investigated by critical Democrats in Congress; see U.S. Congress, House of Representatives, 84th Cong., 2d sess., Committee on the Judiciary, Anti-Trust Subcommittee, *Interim Report . . . on WOCs and Government Advisory Groups* (Washington, 1956).

exchange views with men whose opinions he respected and whose support was indispensable to his broader purposes. He hoped, it seems clear, that such gatherings would also stimulate business leaders to think in broad, cooperative terms and not just according to their more immediate and parochial interests.³⁸

Eisenhower's stress on cooperation and industrial self-government was nowhere better revealed than in his approach to fiscal and monetary policy. Although he believed that the state should play an important role in maintaining prosperity, he also believed in what he called "shared responsibility" between business and government. During the recession of 1954 he privately urged bankers to lower interest rates and make credit more easily available; in 1955 he attempted, unsuccessfully, to persuade auto industry leaders to restrain prices and production; during the recession years of 1954 and 1958 he enlisted the services of the Advertising Council, which launched massive advertising campaigns designed to promote "Confidence in a Growing America," and, though never explicitly stated, confidence in the administration as well; and in 1958–59 he quietly sought to organize corporate leaders behind his wage and price stabilization policies.³⁹

The administration of antitrust policy under Eisenhower was characterized, similarly, by the widespread use of prefiling conferences, consent decrees, and premerger clearances, all of which emphasized cooperation and quiet negotiation. When business complaints of harassment reached the president, moreover, he admonished Attorney General Herbert Brownell, Jr., to reassure the business community as to "the true attitude of this Administration," which was that "continued prosperity and growth of the economy" could come "only through the cooperation of labor, management and government, and that such cooperation requires a readiness of all parties to observe the law (or to seek legislative changes in it) but the avoidance on the part of government of all kinds of petty annoyances brought about merely by personal bias."⁴⁰

³⁸ Dillon Anderson to Eisenhower, January 29, 1954, DDEL, EPP, Administration series, box 8; George Whitney to Eisenhower, July 7, 1953, DDEL, EPP, Name series, box 34; and "Presidential Dinner," November 6, 1953, DDEL, Robinson Papers, box 2. To a fairly typical dinner, held on September 23, 1953, Eisenhower invited, among others, Frank Abrams of Standard Oil, C. F. Craig of A.T.&T., Harlow Curtice of General Motors, Benjamin F. Fairless of U.S. Steel, Henry Ford II of Ford Motor Company, John J. McCloy of the Chase Manhattan Bank, and Richard K. Mellon of Mellon and Sons; Stag Dinner, September 23, 1953, DDEL, EPP, DDE Diaries series, box 2.

³⁹ On bankers and credit, see George Whitney to Eisenhower, June 18, 1954, DDEL, EPP, Name series, box 43; Whitney to George Humphrey, July 27, 1954, DDEL, EPP, Administration series, box 22; and Cabinet Meeting, June 11, 1954, DDEL, EPP, Cabinet series, box 3. On the automobile industry, see George Humphrey to William Mc. Martin, April 24, 1956, DDEL, EPP, Administration series, box 23; and Eisenhower to Arthur Burns, March 20, 1958, *ibid.*, box 10. On the Advertising Council, see James R. Lambie, Jr., to Sherman Adams, December 9, 1953, DDEL, Lambie Records, box 3; Max Fox to Theodore S. Repplier, April 5, 1954, *ibid.*, box 12; Paul West to Sherman Adams, March 12, 1954, *ibid.* Also see Griffith, "The Selling of America," 31–45. Eisenhower also sought to enlist the Advertising Council in the fight against inflation, but with much more limited success. On Eisenhower's efforts to recruit business leaders in the campaign against inflation, see, for example, Eisenhower to Robert Anderson, November 3, 1958, DDEL, EPP, DDE Diaries series, box 32; Eisenhower to Richard K. Mellon, October 6, 1958, *ibid.*, box 22; and Eisenhower to Frank Stanton, February 23, 1959, DDEL, EPP, Administration series, box 39. Eisenhower told Mellon that "only by getting into politics up to our necks can we reverse these unfortunate trends" favored by Americans for Democratic Action and the Congress of Industrial Organizations. On the 1959–60 effort to enlist business support on behalf of wage and price stabilization, also see W. Allen Wallis to Richard M. Nixon, July 29, 1959, DDEL, W. Allen Wallis Records, box 3; Wallis to H. Bruce Palmer, January 20, 1960, *ibid.*, and Progress Report, The Council for Economic Growth and Security, Inc. (January 1960), *ibid.*

⁴⁰ Eisenhower to Brownell, June 12, 1957, DDEL, EPP, Administration series, box 8. For an extremely

The very success of such strategies, of course, depended on the ability and willingness of business leaders to exercise restraint and discipline. Eisenhower fully understood the enormous power of modern business and how decisions made by corporate boards could affect “the whole life of the United States.” He understood also, as he told economic adviser Gabriel Hauge, how such a situation “could be very dangerous unless people act with the greatest wisdom and concert for the nation.”⁴¹ His speeches, his conversations, and his private correspondence are literally filled with appeals for corporate “statesmanship” and responsibility. Nothing so angered him, moreover, as what he considered corporate short-sightedness—the demand by some business groups for tax reduction regardless of the fiscal consequences, the pressure for tariff protection from inefficient producers, the refusal of steel and auto industry leaders to hold down prices, the unwillingness of the automakers to smooth out production and help stabilize the business cycle. When in mid-1955 steel companies announced an increase in steel prices of seven dollars per ton, he told Ann Whitman that he was “pretty disgusted with businessmen and didn’t know when he would get over it.” He was outraged by the heavy-handed lobbying and even bribery that accompanied the drive for deregulation of natural gas and that compelled him to veto a bill he otherwise favored. “I want to give business a honorable place,” he angrily declared, “but they make crooks out of themselves.” To the cabinet he worried aloud about “the contradiction that existed when the greatest exponents of a free economy failed to exercise the restraint necessary to a free economy.”⁴²

Yet even successful instances of corporate cooperation did not, as Eisenhower assumed they would, necessarily produce results that transcended corporate self-interest. In housing and urban affairs, for example, Eisenhower turned for assistance to a national advisory committee recruited for the most part from among realtors, builders, and bankers—groups notoriously hostile to public housing though not to other forms of federal intervention such as FHA and urban renewal. The Housing Act of 1954 and other administration programs, not surprisingly, reflected the political agenda of these powerful groups.⁴³ In the case of the federal

sympathetic, if unpersuasive, account of Eisenhower’s antitrust policies, see Theodore P. Kovaleff, *Business and Government during the Eisenhower Administration: A Study of the Antitrust Division of the Justice Department* (Athens, Ohio, 1980). The administration’s handling of the critical oil cartel case well illustrates its emphasis on negotiation and cooperative arrangements as well as the generally low priority it gave to antitrust enforcement; see Burton I. Kaufman, *The Oil Cartel Case: A Documentary Study of Antitrust Activity in the Cold War Era* (Westport, Conn., 1978), 50–101.

⁴¹ Ann Whitman Diary, February 13, 1956, April 3, 1956, DDEL, EPP, Ann Whitman Diary series, box 8.

⁴² Whitman Diary, June 1, 1955, DDEL, EPP, Ann Whitman Diary series, box 6; Whitman Diary, February 13, March 13, 1956, *ibid.*, box 8; *Eisenhower Diaries* (April 20, 1956), 326; Eisenhower, Telephone conversation with George Humphrey, January 31, 1956, DDEL, EPP, DDE Diary series, box 7; Jack Z. Anderson, Memorandum for the Record, June 19, 1958, *ibid.*, box 20; Cabinet meeting, June 5, 1953, DDEL, EPP, Cabinet series, box 2; Cabinet meeting, December 14, 1956, *ibid.*, box 6; and Memorandum of a Conversation between the President and T. S. Repplier, August 3, 1955, DDEL, Lambie Records, box 23.

⁴³ The Housing Act of 1949 had authorized construction of 135,000 public housing units per year, a rate never reached because of the intervention of the Korean War. Some 58,000 units were constructed in 1953, reflecting commitments entered into during the last years of the Truman administration. By contrast, construction fell off sharply during the Eisenhower years, averaging between 10,000 and 20,000 units per year. By the end of the decade, critics could rightly charge that far more poor people had been displaced through urban renewal than could be housed in newly constructed public housing. See Mark I. Gelfand, A

highway program, Eisenhower decided to name an advisory committee chaired by his close friend Lucius Clay, which he hoped would possess a national view of highway development and would therefore surmount the more parochial interests of truckers, auto clubs, state highway engineers, car manufacturers, oil companies, and others whose inability to reach agreement had deadlocked federal policy for more than a decade. The committee's report did receive broad support, and the bill that passed one year later embodied some, though by no means all, of its major recommendations. The Clay report and the Interstate Highway Act of 1956 also revealed, however, the sharp limitations of corporate stewardship; for, although the Clay committee took a broad, national view, that view was nevertheless narrowly circumscribed by those corporate and professional elites who saw highways solely as a means of moving more automobiles and trucks more cheaply and efficiently. The report, and subsequent Congressional testimony, was silent concerning the impact of such massive highway construction on the ecology of cities, on land use patterns and tax bases, on slum clearance and housing, on renewal and redevelopment, and on urban mass transportation. Thus, the program, though producing enormous growth in the auto, oil, construction, and other related industries, laid an enormous if immeasurable tax upon the American people in the form of disintegrating cities, declining public transportation, air pollution, and wasteful energy consumption.⁴⁴

In natural resource development, Eisenhower sought to replace what he called an "exclusive dependence on Federal bureaucracy" with "a partnership of state and local communities, private citizens, and the Federal Government, all working together." In practice, the "private citizens" involved were almost always large private utilities, owned and controlled by financial interests in New York, Boston, and other metropolitan districts; "local" rural electric cooperatives were actively discouraged, as were "partnerships" between cooperatives and federal power projects. Similarly, the Atomic Energy Act of 1954, which authorized the licensing of private corporations to produce and market nuclear electric power while explicitly prohibiting the Atomic Energy Commission from doing so, made it certain that the future development of this important new resource would be

Nation of Cities: The Federal Government and Urban America, 1933-1965 (New York, 1975), 157-97. Also see Noah P. Mason, "Report to the President on Housing," January 11, 1961, DDEL, EPP, Administration series, box 28. For Eisenhower's views on public housing, which were somewhat more liberal than those of Congressional conservatives, see Cabinet meeting, December 9, 1953, DDEL, EPP, Cabinet series, box 2; Cabinet meeting, December 10, 1954, *ibid.*, box 5; and Legislative Leadership meeting, December 17, 1953, *ibid.*, Legislative series, box 1.

⁴⁴ Both Eisenhower and the Clay Committee had recommended financing highway construction through revenue bonds, but this proposal was blocked in Congress. The final measure incorporated a Democratic-sponsored, "pay as you go" tax on highway users, to be administered through a Highway Trust Fund. Although this approach disappointed economic advisers such as Arthur Burns and John H. Bragdon, who had hoped to use highway construction as a countercyclical tool, it gained support from George Humphrey, Sinclair Weeks, and finally Eisenhower himself. It did, as Eisenhower desired, insulate highway finance from the federal budget. See Legislative Leadership meetings, June 21 and 28, July 6, and December 12, 1955, DDEL, EPP, Legislative series, box 1; and Legislative Leadership meeting, January 31, 1956, *ibid.*, box 2. For critical discussions of the federal highway program, especially see Mark H. Rose, *Interstate: Express Highway Politics, 1941-1956* (Lawrence, Kans., 1979), 95-100; and Gelfand, *A Nation of Cities*, 226-30.

controlled by large, private utilities.⁴⁵ Here, as in other areas, Eisenhower's policies thus decisively shaped the political agenda of future generations.

Eisenhower faced greater difficulties in those areas of public policy, such as agriculture and labor, where the power of corporate elites was contested by other powerful interests. He sought to shape agricultural policy with the cooperation of a national advisory commission representing the powerful triad of large commercial farmers, food processors, and land-grant economists who increasingly dominated national agricultural policy. He believed, as did most of the members of this commission, that the role of government in agriculture should be reduced and that price supports in particular should be lowered. Although the reduction of supports would, he believed, necessitate some "readjustment" in farming ("readjustment" was the then current administrative euphemism for recession), the long-term result would be production for demand and an end to costly surpluses. Eisenhower repeatedly defended the frequently unpopular efforts of Ezra Taft Benson to move in this direction, however much he differed from the tactless agriculture secretary on questions of pace, timing, and public relations. But Congressional leaders, less sanguine than Eisenhower about the social costs of "readjustment" and more sensitive to outcries from the farm belt, frustrated the president. He obtained some reduction of price supports in the farm bills of 1954 and 1958, but the government remained pledged, as it had since the 1930s, to an elaborate system of price supports and production controls. Indeed, even though farm income sharply declined and hundreds of thousands of small farmers were forced from the land, production continued to increase and surpluses to pile up in government warehouses. Eisenhower was convinced that, in agriculture as in other areas, the enemies of good policy were demagogic politicians and greedy partisans of self-interest, especially those farm-state leaders who insisted on the retention of high price supports or who declined to accept his version of the corporate commonwealth. By the end of the decade, however, he clearly realized the dimensions of his own failure, was more willing than before to seek compromise with Congressional leaders, and believed, as he put it, that the "application of . . . principles is not always easy and simple in a society as complex as ours."⁴⁶

In the case of labor, Eisenhower's views and policies were divided. On the one

⁴⁵ *Congress and the Nation, 1945-1964*, 832-966. For opposition to the administration's power policies, see National Rural Electric Cooperative Association, "Washington Report," September 1953, HSTL, Wallace J. Campbell Papers, box 31; and James G. Patton, "Call for a National Electric Consumers' Conference," December 10-11, 1953, *ibid.*, box 40. For a critical analysis of the administration's atomic energy legislation, see Leland Olds to American Public Power Association *et al.*, August 9, 1954, Library of Congress, Clinton P. Anderson Papers, box 791; and National Rural Electric Cooperative Association, "The Atomic Energy Act of 1954," HSTL, Harry S. Truman Papers, Post-Presidential Name File, box 4.

⁴⁶ *Eisenhower Public Papers* (1953), 1: 28, 530, 574; Eisenhower, Memorandum for the Secretary of Agriculture, September 8, 1954, DDEL, EPP, Administration series, box 6; Legislative Leadership meeting, March 18, 1958, *ibid.*, Legislative series, box 2; and Eisenhower, *Mandate for Change: The White House Years, 1953-56* (Garden City, N.Y., 1963), 557-64. For Eisenhower's critical comments on the politics of agriculture, see *Eisenhower Diaries* (February 11, 1956), 317-18; L. Arthur Minnich, Staff Note, June 21, 1954, DDEL, EPP, DDE Diaries series, box 4; Eisenhower to Joel Carlson, April 5, 1956, *ibid.*, Name series, box 4; and Eisenhower to Benson, March 20, 1958, *ibid.*, Administration series, box 6. For a partisan critique of the Eisenhower program, see Wesley McCune, *Who's Behind Our Farm Policy?* (New York, 1956). For a defense of the Benson policies, see Edward L. Schapsmeier and Frank H. Schapsmeier, *Ezra Taft Benson and the Politics of Agriculture: The Eisenhower Years, 1953-1961* (Danville, Ill., 1975).

hand, he accepted the existence of labor unions as an unavoidable part of the nation's corporate order; on the other, he worried over what he saw as their divisive appeals to class selfishness and greed and over the possibility that the wage settlements they compelled might lead to a ruinous inflation. Nor could he bring himself to accord to labor leaders the same degree of legitimacy he bestowed upon the leaders of business and industry. In his diary he confessed to "disappointment" over his first secretary of labor, plumbers' union president Martin Durkin, who "could never free himself of the feeling that he was placed in the Cabinet to be a trade unionist." He was far more comfortable with Durkin's successor, James P. Mitchell, who had handled personnel and industrial relations for Macy's and Bloomingdale's and whose views and style more closely approximated his own. To his brother Milton he confided that, while his labor policies were designed to appeal to the mass of American workers, "most certainly" his administration was "not consciously seeking the favor of the so-called labor leader." In dealing with labor Eisenhower repeatedly returned to the themes of cooperation and mutuality that had characterized his thinking since at least the late 1940s. "The President," an aide recorded in early 1953, "made an eloquent presentation of the need for cooperation by free men . . . in order to make Democracy work. He emphasized the need for a cooperative climate rather than rigid specification by law." The foundations of the administration's labor philosophy, concluded Secretary Mitchell, were "cooperation, mutual understanding, self-respect and respect for other viewpoints, restraint and self-discipline."⁴⁷

During the 1952 campaign Eisenhower had promised to seek changes in the Taft-Hartley law, only to be subsequently caught in a bitter struggle between the unions, who hoped to eliminate some of the law's antilabor features, and business leaders, who wanted even more restrictive legislation. He never permitted himself to become too closely identified with Taft-Hartley revision, however, and he quickly abandoned efforts to change the law once it became clear how difficult and politically costly this would be.⁴⁸ Four years later, following Senate revelations of corruption in labor unions, he again called for labor legislation. This time, however, he played an extremely active and skillful role in winning passage of the Landrum-Griffin Act, a measure that generally reflected the wishes of corporate lobbyists.⁴⁹

⁴⁷ *Eisenhower Diaries* (January 19, 1954), 267; Eisenhower to Milton Eisenhower, January 6, 1954, DDEL, EPP, DDE Diaries series, box 3; Eisenhower, Conversation with Sinclair Weeks, November 7, 1953, DDEL, EPP, Administration series, box 42; Cabinet meeting, February 20, 1953, DDEL, EPP, Cabinet series, box 1; and Mitchell, "The Accomplishments of the Department of Labor, 1953-1961," DDEL, EPP, Administration series, box 29. Efforts to create a tripartite (labor, business, and the administration) advisory committee foundered on conflict between business and labor representatives; Legislative Leadership meeting, March 9, 1953, DDEL, EPP, DDE Diaries series, box 3.

⁴⁸ On administration attempts to resolve the conflict, see Bernard Shanley and Jerry Morgan, Memorandum for the President, September 20, 1953, DDEL, EPP, Administration series, box 41. On the opposition of business leaders, see Philip D. Reed to Eisenhower, December 23, 1953, *ibid.*; Lewis W. Douglas to Eisenhower, December 16, 1953, *ibid.*, box 13; Eisenhower to Clarence Francis, December 19, 1953, DDEL, EPP, DDE Diaries series, box 2; Memorandum of Conversation with Roy Roberts, December 11, 1953, *ibid.*; and Cabinet meeting, May 13, 1955, DDEL, EPP, Cabinet series, box 5.

⁴⁹ Summary Statement of the President's Proposals . . . [ca. November 8, 1957], DDEL, EPP, Cabinet series, box 10; and Cabinet meeting, November 8, 1957, *ibid.* The extraordinary role of the White House is detailed in the minutes of the president's meetings with Congressional leaders. See notes on meetings with legislative leaders, December 4, 1957-September 8, 1959, DDEL, EPP, Legislative series, box 3. Also see

Eisenhower sought, with some success, to limit the direct and formal role of the White House in labor conflict. He invoked Taft-Hartley far less frequently than had Truman, preferring to work indirectly and behind the scenes. When he did invoke the law, moreover, as he did during the long steel strike of 1959, he did so very reluctantly. The weight of White House intervention, more often than not, worked against high wage settlements that might, in the administration's eyes, prove inflationary. Thus in early 1960 White House leaders congratulated themselves for having achieved a steel settlement that was lower than those obtained in the can and aluminum industries and that, had the rank and file been allowed to vote, would have been rejected by an overwhelming margin. The administration also sought to encourage industry-wide bargaining and the negotiation of long-term contracts, thereby increasing industrial stability. The result, though Eisenhower could scarcely claim full credit, was a remarkable period of industrial tranquility during which, in comparison with the Truman years, there were far fewer strikes, far fewer workers involved, and far fewer workdays lost. Real wages improved somewhat during the decade, and most workers enjoyed a degree of security that stood in sharp contrast to their recollections of the 1930s. Yet these gains were modest, and large numbers of Americans, one-fifth or more of the population, remained in poverty.⁵⁰ Thus while labor and the laboring classes became partners in the corporate commonwealth, they remained very junior members at best.

THE SEARCH FOR STABILITY AND SOCIAL HARMONY underlying Eisenhower's vision of a corporate commonwealth also shaped his style of presidential leadership. This style was quite obviously a product of his experience as a military leader and of his conviction that modern government was so large and complex that no individual could master all of its intricacies. It was clearly reinforced by the practical necessity of working for the most part with a Congress organized by the opposing party. It was also shaped, however, by the conservative and consensual goals of his presidency and by his belief in the limited role of the political state, in the dangers of popular politics, in the importance of persuasion rather than coercion, and in the necessity for voluntary discipline, restraint, and cooperation among America's powerful economic groupings. Its purpose was to deflect attention not only from himself but also from the national government itself, to deflate and depoliticize

Alan K. McAdams, *Power and Politics in Labor Legislation* (New York, 1964), 71–74, 272–73: “The distinctive feature of the management side of the labor reform battle was the degree of participation by the White House,” concludes this study of the bill’s passage. “The direct coordination of the management groups by the Administration made possible working relationships which were smoother than had ever been achieved before among usually widely ranging groups.”

⁵⁰ Cabinet meetings, April 3, December 15, 1953, DDEL, EPP, Cabinet series, box 2; Cabinet meeting, November 11, 1954, *ibid.*, box 4; and Allen Wallis, Memorandum for the President, August 7, 1954, DDEL, Wallis Records, box 1. On the steel strike, especially see the notes on Eisenhower’s separate meetings with industry and labor leaders, September 30, 1959, DDEL, EPP, DDE Diaries series, box 28; Summary of Steel Dispute, DDEL, Hagerty Papers, box 10; Eisenhower, Memorandum for the Record, November 9, 1959, DDEL, EPP, DDE Diaries series, box 11; Eisenhower, Telephone Conference Call with Richard M. Nixon, James P. Mitchell, Wilton B. Persons, January 2, 1960, *ibid.*, box 30; and Legislative Leadership meeting, January 12, 1960, DDEL, EPP, Legislative series, box 3. Also see *Congress and the Nation, 1945–1964*, 628–29, *passim*; and Harold G. Vatter, *The U.S. Economy in the 1950’s: An Economic History* (New York, 1963), 221–48.

expectations raised by two decades of Democratic rule. Thus, he sought to govern by indirection, delegating authority to those he trusted and with whom he was in basic agreement, but insulating himself from the controversy and criticism their actions might provoke. He tried quite deliberately to appear above partisan politics, refused to be drawn into personal confrontations, and almost never displayed in public his legendary temper. To those around him, he repeatedly counseled moderation and restraint. The task of the political leader, he wrote in a long letter to Nelson Rockefeller, was “to devise plans along which humans [can] make constructive progress. This means that the plan or program itself tends to fall in the ‘gray’ category even though an earnest attempt is made to apply the black and white values of moral truths. . . . Perfection is not quickly reached; the plan is therefore ‘gray’ or ‘middle-of-the-road.’”⁵¹

His experience as president also reinforced his distrust of popular democracy. People in the aggregate, he seemed to believe, were all too prone to self-seeking and all too vulnerable to the blandishments of demagogues. Congress was a warren of greedy special interests (an “occupational hazard,” one aide quipped), while the press was little more than an endless source of “distortion and gross error.”⁵² Mistrustful of democracy, he opposed most efforts to modify the nation’s constitutional arrangements. He believed that the Bricker Amendment would undermine the foundations of presidential authority in foreign affairs, and that attempts by Henry Cabot Lodge and others to change the electoral system by introducing proportional voting would make the American system “closer to a democracy, less of a republic.” “We can’t let just a popular majority sweep us in one direction,” he told Vice President Nixon, “because then you can’t recover.” He defended the Supreme Court not because he always agreed with its decisions, but because he believed that “one of the great functions of the Supreme Court was to provide needed stability in a form of government where political expediency might at times carry parties and political leaders to extremes.”⁵³

Fear of popular politics and a commitment to voluntarism and corporate self-

⁵¹ For discussion of Eisenhower’s leadership by those who worked with him, especially see Emmet John Hughes, *The Ordeal of Power: A Political Memoir of the Eisenhower Years* (New York, 1963), 123–27; and Larson, *The President Nobody Knew*, 12–33. Also see Parmet, *Eisenhower and the American Crusades*, 175, 209–10. For the best and most recent appraisal of Eisenhower’s leadership, see Fred I. Greenstein, “Eisenhower as an Activist President,” *Political Science Quarterly*, 94 (1979–80): 575–99. For Eisenhower’s own reflections on leadership, see especially Eisenhower to Emmet John Hughes, December 10, 1953, DDEL, EPP, DDE Diaries series, box 2; Eisenhower to Milton Eisenhower, October 9, 1953, *ibid.*; Eisenhower to Rockefeller, May 5, 1960, *ibid.*, box 32; Eisenhower to Henry Luce, August 8, 1960, *ibid.*, box 33; and Eisenhower to William Phillips, June 5, 1953, DDEL, EPP, Name series, box 25.

⁵² On the public, see, for example, Eisenhower to Hughes, December 10, 1953; and Eisenhower to Lamar Fleming, Jr., December 5, 1958, DDEL, EPP, DDE Diaries series, box 23. For typical remarks on Congress, see Eisenhower to Everett Hazlett, July 22, 1957, DDEL, EPP, DDE Diaries series, box 15; and Eisenhower to Arthur Burns, March 11, 1959, DDEL, EPP, Administration series, box 10. The White House aide is quoted in the Whitman Diary, August 11, 1954, DDEL, EPP, Ann Whitman Diary series, box 2. On the press, see—again for fairly typical expressions—*Eisenhower Diaries* (January 19, 1954), 270–72; and Eisenhower to Edgar Eisenhower, January 27, 1954; Eisenhower to Everett Hazlett, April 27, 1954, DDEL, EPP, DDE Diaries series, box 4; and Eisenhower to Robinson, August 4, 1958, *ibid.*, box 21.

⁵³ On the Bricker Amendment, see Eisenhower to Edgar Eisenhower, December 12, 1954, DDEL, EPP, DDE Diaries series, box 3; Eisenhower to J. Earl Schaeffer, January 22, 1954, *ibid.*; and Eisenhower to John

government also shaped Eisenhower's preoccupation with public relations: if he hoped to avoid coercive state intervention and to encourage the resolution of conflict among powerful interests, his chief techniques had to be persuasive; and if the principle threat to this process arose from lack of discipline among the masses and the demagogic promises of politicians, then public relations must play an even more important role in encouraging restraint, defusing dangerous issues, dampening protest and legitimizing corporate rule. The administration faced problems "not unlike the advertising and sales activity of a great industrial organization," he noted in 1953; and, while it was necessary to have "a good product to sell," it was also necessary "to have an effective and persuasive way of informing the public of the excellence of that product."⁵⁴ He established a standing committee on public relations within the White House, followed closely the public relations efforts of the Republican national committee, and maintained a steady correspondence with friends and advisors from the world of corporate public relations. He directed the organization of special campaigns on agriculture, highways, public power, labor relations, and economic policy, and he enlisted the support of powerful private groups such as the Advertising Council in publicizing his policies.⁵⁵ He was exceptionally skillful in his press relations. He maintained a wide correspondence with many publishers and editors, invited them frequently to his stag dinners, and even employed them on occasion to conduct confidential surveys on his behalf. He was extremely sensitive to adverse comment in the press and frequently sought to counter such criticism, though almost always obliquely.⁵⁶

J. McCloy, January 19, 1954, *ibid.* On electoral college reform, see the Whitman Diary, March 20, 1956, DDEL, EPP, Ann Whitman Diary series, box 8; and Pre-legislative Leadership meeting, March 20, 1956, DDEL, EPP, Legislative series, box 2. On the Supreme Court, see Eisenhower to E. F. Hutton, July 10, 1957, DDEL, EPP, DDE Diaries series, box 14. Those changes that Eisenhower did support were almost invariably those that would have strengthened the executive or further insulated political leaders from popular influence: for example, the item veto, the requirement of a two-thirds vote to reject presidential nominations, and four-year terms for Congress; Legislative Leadership meeting, September 9, 1959, DDEL, EPP, Legislative series, box 3.

⁵⁴ Eisenhower to George Humphrey, Arthur Summerfield, Henry Cabot Lodge, Sherman Adams, *et al.*, November 23, 1953, DDEL, EPP, Administration series, box 33.

⁵⁵ Cabinet meeting, July 3, 1953, DDEL, EPP, Cabinet series, box 2; Cabinet meeting, March 5, 1954, *ibid.*, box 3; Cabinet meeting, November 5, 1954, *ibid.*, box 4; and Cabinet meeting, July 8, 1955, *ibid.*, box 5. One of the president's sharpest and most frequently proffered criticisms was on the lack of public relations skill among his subordinates. See, for example, Eisenhower to Charles E. Wilson, November 2, 1953, DDEL, EPP, Administration series, box 44; and Eisenhower, Memorandum for the Files, March 12, 1955, DDEL, EPP, Ann Whitman Diary series, box 4. For Eisenhower's own concern with public relations, see, among others, Eisenhower to Sigurd S. Larmon, February 1, 1954, DDEL, EPP, DDE Diaries series, box 3; Eisenhower to William E. Robinson, August 4, 1954, *ibid.*, box 4; Eisenhower, Memorandums for Robert Montgomery, March 12 and August 18, 1954, DDEL, EPP, Administration series, box 29; and Eisenhower, Memorandum of Appointment [with Sig Larmon, of Young & Rubicam, Kenneth Dyke of Batten, Barton, Durstine and Osborne, and others], September 2, 1955, DDEL, EPP, Name series, box 20. For public relations in agriculture, see, for example, Belknap to Secretary of Agriculture, December 9, 1953, DDEL, Benson Papers, Public Relations, roll 27. On the role of public relations in economic policy, see Eisenhower to Raymond Saulnier, February 11, 1958, DDEL, EPP, Administration series, box 35. On public power, see Sherman Adams, Memorandum for Howard Pyle, June 1, 1955, DDEL, Pyle Records, box 37. On highways, see Whitman Diary, February 12, 1955, DDEL, EPP, Ann Whitman Diary series, box 4. On labor relations, see McAdams, *Power and Politics in Labor Legislation*, 74–75, 120, 178. And, on the Advertising Council, see Griffith, "The Selling of America," 28–47.

⁵⁶ On Eisenhower's use of newspaper editors in conducting confidential surveys, see Cabinet meeting,

Eisenhower did not, however, limit these efforts, which he frequently referred to as “selling the American people,” to advertising his highly marketable personality or even to promoting the specific programs of the administration but directed them as well toward the broader, long-term goals of his presidency—winning popular acceptance of the discipline and self-restraint necessary to the corporate commonwealth and helping “our people understand that they must avoid extremes in reaching solutions to the social, economic and political problems that are constantly with us.”⁵⁷ His approach to these broad aims was nowhere better illustrated than in the creation, near the end of his second term, of a commission to identify and publicize national goals for the 1960s. Chief among these goals, Eisenhower made clear in advance, was the “American aspiration . . . to develop a world in which all peoples will be living at peace under cooperative policies with maximum standards of living and opportunity for all.” The most important purpose of the study, he noted privately, “was to outline for the American people problems involved in mobilizing public opinion in a democracy in order to make the hard decisions that would be needed to successfully compete in an indefinite cold war.”⁵⁸

Eisenhower’s deep concern for public order and consensus influenced his response to Joseph R. McCarthy and the discordant Cold War politics of anticommunism. While he repeatedly insisted on the importance of justice and fair play, he tended, almost without exception, to resolve conflicts between security and civil liberties in favor of the state. He supported legislation to strip citizenship from those convicted under the Smith Act of conspiring to advocate the violent overthrow of the government, to compel witnesses to testify in national security investigations, to legalize the use of wiretap information in internal security cases,

February 16, 1954, DDEL, EPP, Cabinet series, box 3; and Roy Roberts to Eisenhower, February 18, 1954, DDEL, EPP, Administrative series, box 21. For specific efforts to shape coverage and editorials, see, for example, Eisenhower, Telephone conversation with Arthur Hays Sulzberger, September 25, 1956, DDEL, EPP, DDE Diaries series, box 10; Eisenhower, Telephone conversation with William E. Robinson, February 6, 1957, *ibid.*, box 8; Gardner Cowles to Eisenhower, April 2, 1956, *ibid.*; Eisenhower to Cowles, April 3, 1956, *ibid.*; Eisenhower to Alfred M. Gruenther, January 15, 1958, *ibid.*, box 18; and Eisenhower to John S. Knight, October 6, 1954, DDEL, EPP, Name series, box 20. On efforts to clean up *Fortune*’s handling of an administration scandal, see C. D. Jackson to Sherman Adams, July 22, 1955, DDEL, C. D. Jackson Papers, box 12. On the seeming indifference of the press to Republican scandals generally, see Frier, *Conflict of Interest in the Eisenhower Administration*, 9–10, 51–52, 89. On the administration’s successful effort to kill a *Denver Post* story on the production of nerve gas, see Special Staff Note, April 1, 1957, DDEL, EPP, DDE Diaries series, box 13. On Eisenhower’s efforts to place a defeated Republican senator at CBS, where he might “insure key television appearances for more good Republican speakers,” see Ed McCabe, Memorandum for Ann Whitman, December 18, 1958, *ibid.*, box 23. “As you know, Spyros Skouras is trying to publicize a few of our younger Republicans by keeping them on the newsreels whenever they attend some ceremony or other”; Eisenhower to Adams, October 29, 1957, *ibid.*, box 16.

⁵⁷ Eisenhower to Everett Hazlett, March 2, 1956, DDEL, EPP, DDE Diaries series, box 8.

⁵⁸ Memorandum Concerning the Commission on National Goals, February 7, 1960, in *Eisenhower Public Papers*, 8: 159–60; and Robert Merriam, Memorandum for the Record, March 19, 1959, DDEL, EPP, DDE Diaries series, box 25. For the commission’s report, see The American Assembly, *Goals for Americans: The Report of the President’s Commission on National Goals* (Englewood Cliffs, N.J., 1960), 1–31. Not surprisingly, the report reflected the president’s own belief in a limited federal state (“Government participation in the economy should be limited to those instances where it is essential to the national interest and where private individuals or organizations cannot adequately meet the need”), in cooperation between business and labor, in a free market for agriculture, in economic growth and modernization, in an “open and peaceful world” characterized by free trade and mutual interdependence, and his conviction that the American people should be summoned to “extraordinary personal responsibility, sustained effort, and sacrifice.”

and to broaden and redefine espionage and sabotage laws. One of his earliest actions as president was to institute a drastic new internal security program that broadened the criteria for federal employment to include not only loyalty and security but also "suitability," abolished the hearing and review procedures established by the Truman administration, and extended the power of summary dismissal, previously reserved to heads of sensitive departments such as state and defense, to all federal agencies and departments. He continued the Truman administration's prosecution of Communist leaders under the Smith Act and approved, in outline at least, the FBI's covert and extralegal COINTELPRO efforts "to promote disruption within the ranks of the Communist Party." He was well aware, moreover, of the FBI's euphemistically labeled "custodial detention" program and was prepared to order suspected subversives rounded up in time of national emergency.⁵⁹ Although he believed that little new evidence had been produced to implicate J. Robert Oppenheimer and that the case had been "constantly reviewed and reexamined over a number of years," Eisenhower nevertheless quickly ordered the famous physicist's security clearance lifted and later defended the AEC's finding that Oppenheimer was a security risk. And, although he considered commuting the sentences of Julius and Ethel Rosenberg, he finally decided, as he wrote his son, that "the exemplary feature of the punishment, the hope that it would deter others, is something that cannot be ignored."⁶⁰

Although Eisenhower had himself on occasion employed the communist issue for political purposes, he nevertheless loathed McCarthy and was sharply critical of the highly publicized investigations in Congress. What he objected to most strenuously in the Congressional proceedings, however, was not so much their arbitrary violation of individual liberties but rather the disorderly and partisan atmosphere in which they were conducted. The job of routing subversives, he believed, was primarily administrative, not legislative—a task for orderly and bureaucratic resolution, not partisan debate. His strongest and most direct stand against the Congressional inquisitors came in the spring of 1954, when he invoked the doctrine of executive privilege in order to protect the privacy of advice offered within the executive branch. He bluntly told Congressional leaders, "Any man who testified as to the advice he gave me won't be working for me that night. . . . I will

⁵⁹ Athan Theoharis, *Spying on Americans: Political Surveillance from Hoover to the Huston Plan* (Philadelphia, 1978), 54, 55, 82–83, 107–10, 155–65, 209–18; and *Congress and the Nation, 1945–1964*, 1656–60. The president's program, boasted Walter Bedell Smith, would have the effect of outlawing the Communist party "without becoming involved in the constitutional complications of actual outlawry." Cabinet meeting, April 2, 1953, DDEL, EPP, Cabinet series, box 2. On rounding up political subversives, see Whitman Diary, June 25, 1954, DDEL, EPP, Ann Whitman Diary series, box 2.

⁶⁰ On Oppenheimer, see *Eisenhower Diaries* (December 2, 3, 1953), 259–60; and Hagerty Diary, May 29, June 1, 10, 1954, DDEL, Hagerty Papers, box 1. The strongest evidence against Oppenheimer, in Eisenhower's view, was the fact that he continued, through 1953, to visit socially with Haakon Chevalier, long after the 1943 "kitchen conversation" in which Chevalier had initiated a discussion about supplying technical information to the Soviet Union; Whitman Diary, June 25, 1954, DDEL, EPP, Ann Whitman Diary series, box 2. On the Rosenberg case, see Eisenhower to John S. D. Eisenhower, June 16, 1953, DDEL, EPP, DDE Diaries series, box 2; and Eisenhower to Clyde Miller, June 10, 1953, DDEL, EPP, Administration series, box 35. Eisenhower was impressed by trial judge Irving R. Kaufman and subsequently sought to elevate him to the U.S. Court of Appeals; Eisenhower, Telephone conversation with Herbert Brownell, February 22, 1955, January 27, 1957, DDEL, EPP, DDE Diaries series, boxes 5, 12.

not allow people around me to be subpoenaed and you might just as well know it now.”⁶¹

He refused to be drawn into a direct confrontation with McCarthy, however, despite repeated entreaties from friends and advisors who feared that the senator’s continued depredations would undermine the president’s leadership. This reluctance arose in part from his realistic, if cynical, respect for McCarthy’s support among Senate Republicans, in part from his personal dislike for the philippic mode. It also derived, however, from his sophisticated analysis of the relationships between McCarthy, the media, and the presidency. McCarthy, he wrote, owed “his entire prominence and influence . . . to the publicity media of the nation.” The president, on the other hand, also possessed a “terrific headline value.” He noted to his friend Hazlett that, “whenever the President takes part in a newspaper trial of some individual of whom he disapproves, one thing is automatically accomplished. This is an increase in the headline value of the individual attacked.”⁶² He chose instead to combat McCarthy through indirection: he urged Republican senators to attack him, ordered a reluctant Richard M. Nixon into combat in order to prevent McCarthy from monopolizing network television, encouraged Paul Hoffman and others to organize an anti-McCarthy movement, prevented McCarthy from addressing party gatherings, and suggested, with great circumspection, that publishers and media executives resist the senator’s demands for time and space. He even suggested—only half jokingly—that since McCarthy had been built up by the press, the press should “develop a collusion to ignore him.”⁶³ In the end, of course, the Senate did act, however reluctantly, in censuring the senator from Wisconsin. Eisenhower remained publicly aloof from the controversy, the press began to ignore McCarthy, and a measure of tranquility returned to American politics. By his own terms, if not by those of liberals or civil libertarians, the president had succeeded in bringing an era to an end.

The sharpest challenge to Eisenhower’s quest for consensus, however, and the one that revealed most clearly the class and racial bias of his ideology, was the struggle by black Americans for civil rights and economic justice. Like most men of power, Eisenhower fully subscribed to the hierarchical values of corporate America. Though he believed in the principle of equality of opportunity, he also subscribed

⁶¹ Eisenhower to Charles E. Wilson, May 17, 1954, in *Eisenhower Public Papers*, 2: 438–84. “I’ve gone to utmost lengths to be cooperative with Congress,” Eisenhower told Republican majority leader William Knowland. “I have declined to get into this mess even when I have been needed by the press, but this is one thing I will fight with all my power—I will not have my men subpoenaed.” Hagerty Diary, May 17, 1954, DDEL, Hagerty Papers, box 1.

⁶² Eisenhower to Everett Hazlett, July 21, 1953, DDEL, EPP, DDE Diaries series, box 2; Eisenhower to William E. Robinson, March 23, 1954, *ibid.*, box 3; Eisenhower to John Reagan McCrary, Jr., December 4, 1954, *ibid.*, box 4; Eisenhower to Philip D. Reed, June 17, 1953, DDEL, EPP, Administration series, box 32; and Eisenhower to Paul H. Helms, March 9, 1954, DDEL, EPP, Name series, box 18.

⁶³ Hagerty Diary, May 20, 29, 30, 1954, DDEL, Hagerty Papers, box 1; Eisenhower, Telephone conversation with Nixon, March 8, 1954, DDEL, EPP, DDE Diaries series, box 3; and Paul G. Hoffman to Sherman Adams, November 26, 1954, HSTL, Hoffman Papers, box 84. Hoffman was one of the key supporters of the campaign to censure McCarthy in 1954; see Robert Griffith, *The Politics of Fear: Joseph R. McCarthy and the Senate* (Lexington, Ky., 1970), 228, 279–81. Hoffman also sought—together with former White House aide and Time-Life executive C. D. Jackson, columnist Roscoe Drummond, and others—to

to its less frequently stated corollary—that such opportunity inevitably created inequality of condition. He shared many of the conventional prejudices common among upper-middle class white Americans toward blacks and other minorities. He believed in equality before the law but not in “social equality.” He did not think, he told Arthur Larson, that everyone had to mingle socially “or that a Negro should court my daughter.”⁶⁴ These attitudes shaped his response to the emerging racial crisis of the 1950s and reinforced the fundamentally conservative elements of his political philosophy: his narrow construction of what was permissible and desirable for the national government to do, his fear of popular passion and his distrust of politics, his preference for cooperation over coercion, and his tendency to insulate the presidency from controversial issues. As president, he opposed the establishment of a Federal Fair Employment Practices Commission as well as any efforts that might project the national government any deeper into the school desegregation controversy opened up by the Brown decision in 1954. Not until 1956 did he call for civil rights legislation, and then only at the insistence of Attorney General Brownell. His support was limited, moreover, to the area of voting rights, where federal responsibility seemed clear, and to the creation of a bipartisan commission to study the problem. The proposed legislation, he assured Senate Majority Leader Lyndon B. Johnson, represented “the mildest civil rights bill possible”; even so, the measure was drastically weakened before enactment in 1957. Three years later, prodded by his own Civil Rights Commission, he again called for legislation, this time an extremely modest proposal that became the Civil Rights Act of 1960.⁶⁵

In civil rights, as in other areas, he preferred to act administratively, without widespread publicity, and where federal jurisdiction was uncontested—for example, in the desegregation of navy yards and the integration of public facilities in the District of Columbia. He also sought, privately, to persuade prominent Southerners

mount a public and press relations campaign aimed at “replacing, in the public mind, the disuniting symbol of McCarthyism with the unifying image of the President as the effective instrument of anti-subversion”; Hoffman to Eisenhower, April 30, 1954, HSTL, Hoffman Papers, box 27. On Eisenhower’s efforts to influence the press, see William S. Paley to Eisenhower, May 22, 1954, DDEL, EPP, Name series, box 25; Eisenhower to Gabriel Hauge, September 30, 1954, DDEL, EPP, DDE Diaries series, box 4; Whitman Diary, April 27, 1954, DDEL, EPP, Ann Whitman Diary series, box 2; William E. Robinson to Eisenhower, July 22, 1953, DDEL, EPP, Name series, box 29; and Eisenhower to Robinson, July 27, 1953, DDEL, EPP, DDE Diaries series, box 3. Many of the liberal businessmen who had supported Eisenhower in 1952 were extremely critical of McCarthy; for example, Harry A. Bullis, Philip D. Reed, and Paul H. Helms. It is especially interesting to note that among the most important contributors to McCarthy’s downfall were three of the founders of the Committee for Economic Development—Democratic Senator William Benton, Republican Senator Ralph Flanders, and Hoffman.

⁶⁴ Larson, *The President Nobody Knew*, 124–33; and Lyon, *Eisenhower: Portrait of the Hero*, 556–57. “Maybe the President might get a chuckle out of this,” observed his close friend Robinson, enclosing a racial joke; William E. Robinson to Ann Whitman, May 10, 1955, DDEL, EPP, Name series, box 27.

⁶⁵ On the 1957 law, especially see Legislative Leadership meetings, April 17, 1956, July 9–August 27, 1957, DDEL, EPP, Legislative series, box 2; Cabinet meeting, March 9, 1956, DDEL, EPP, Cabinet series, box 6; Cabinet meeting, March 23, 1956, *ibid.*, box 7; Cabinet meeting, August 2, 1957, *ibid.*, box 9; and Eisenhower, Telephone conversation with Lyndon B. Johnson, June 15, 1957, DDEL, EPP, DDE Diaries series, box 14. For Eisenhower’s misgivings, see Gerald D. Morgan, Memorandum for the Record, March 24, 1956, *ibid.*, box 8. On the 1960 law, see Legislative Leadership meetings, February 2, April 26, 1960, DDEL, EPP, Legislative series, box 3. For a chronology of civil rights legislation, see *Congress and the Nation, 1945–1964*, 1621–30. For an excellent legislative history of the Eisenhower program, see Steven F. Lawson, *Black Ballots: Voting Rights in the South, 1944–1969* (New York, 1976), 140–249.

to embrace his own goals of moderation and gradual progress.⁶⁶ He feared the passions aroused by civil rights, both among blacks and Southern whites, and repeatedly preached patience, calmness, and forbearance. As he told Booker T. Washington's daughter, "I like to feel that where we have to change the hearts of men, we cannot do it by cold lawmaking, but must make these changes by appealing to reason, by prayer, and by constantly working at it through our own efforts."⁶⁷ He insisted, moreover, on insulating himself from the actions of the Supreme Court, the Civil Rights Commission, and even his own attorney general. Thus, in the school desegregation cases he carefully avoided identification with Attorney General Brownell, whose *amicus curiae* brief had drawn sharp criticism from Southern conservatives. He repeatedly refused to endorse the Brown decision or to identify himself publicly with the goal of desegregation. Privately, he thought the decision a mistake that would set back racial progress throughout the South. Desegregation, he believed, would require over thirty to forty years to complete.⁶⁸ When efforts at conciliation failed and he was compelled, however reluctantly, to dispatch federal troops to Little Rock, Arkansas, he carefully couched his actions in terms of defending civil order, not civil rights. As he explained to his friend Hazlett, "My biggest problem has been to make people see . . . that my main interest is not in the integration or segregation question. My opinion as to the wisdom or timeliness of the Supreme Court's decision has nothing to do with the case. . . . If the day comes when we can obey the orders of our Courts only when we personally approve of them, the end of the American system . . . will not be far off." In civil rights, as in other areas, a concern for order and stability predominated; and it was the president's firm intention, as he told South Carolina Governor James F. Byrnes, "to make haste slowly."⁶⁹

EISENHOWER'S QUEST FOR A CORPORATE COMMONWEALTH at home was paralleled and inextricably bound to the struggle to create, at least among the so-called free nations, an interdependent and cooperative world order. Like other American leaders he believed that freedom, security, and prosperity were indivisible and that little domestic progress was possible in the absence of an international "atmosphere

⁶⁶ Larson, *The President Nobody Knew*, 124–33; Staff meeting, April 9, 1953, DDEL, EPP, DDE Diaries series, box 2; Eisenhower, Speech to the NAACP, March 10, 1954, in *Eisenhower Public Papers*, 2: 310; Eisenhower to James F. Byrnes, August 14, 1953, December 1, 1953, DDEL, EPP, DDE Diaries series, box 3; Eisenhower to Billy Graham, March 22, 1956, *ibid.*, box 8; Eisenhower to Ralph McGill, November 4, 1957, *ibid.*, box 17; Eisenhower to Billy Graham, March 30, 1956, DDEL, EPP, Name series, box 16; and Eisenhower to C. C. Warren, March 30, 1956, DDEL, White House Central Files, O.F. 141-B-1, box 730.

⁶⁷ E. Frederick Morrow, *Black Man in the White House: A Diary of the Administrative Years* (New York, 1963), 98.

⁶⁸ Eisenhower to James F. Byrnes, December 1, 1953, DDEL, EPP, DDE Diaries series, box 3; Maxwell M. Rabb, Memorandum for Governor Adams, November 12, 1957, DDEL, E. Frederick Morrow Records, box 10; Eisenhower, Telephone conversation with William P. Rogers, August 22, 1958, DDEL, EPP, Administration series, box 35; Larson, *The President Nobody Knew*, 124–26; and Hughes, *The Ordeal of Power*, 200–01. In 1956, Eisenhower demanded that the Republican platform committee excise any words to the effect that the Eisenhower administration supported the Court's decision; Eisenhower, Telephone conversation with Herbert Brownell, August 19, 1956, DDEL, EPP, Ann Whitman Diary series, box 8.

⁶⁹ Eisenhower to Everett Hazlett, November 8, 1957, DDEL, EPP, Name series, box 18; and Eisenhower to Byrnes, July 23, 1957, DDEL, EPP, DDE Diaries series, box 14.

in which America can be safe and prosperous.” The challenge to such a system, he believed, was threefold: most obviously from the Soviet Union and other Communist nations but also from the Western nations, which might unthinkingly allow the world to fall victim to communism because each was “too preoccupied with its own local and selfish interests,” and from within the United States itself, where greedy pressure groups might undermine long-run national and international interests. If, on the one hand, a disorderly and dangerous world could disrupt America's future progress and prosperity, so, on the other hand, greed and shortsightedness at home could undermine American goals abroad.⁷⁰ The purpose of foreign policy therefore lay in the mastery of these contradictions. This meant, to begin with, convincing Western nations that their (and America's) long-run interests demanded cooperation and mutual restraint. Too often, he believed, such cooperation was sacrificed to what he considered parochial interests and loyalties: the Arab-Israeli conflict in the Middle East, the Indian-Pakistani struggle over Kashmir, Korean antagonism toward Japan, and the unwillingness of European colonial powers to yield their prewar empires, to cite some of the examples he most frequently used in his private correspondence.

Sound foreign policy and broad, long-term national interests also meant that it was often necessary to restrain domestic interests. Expanded international trade, for example, which he considered absolutely vital to American and world prosperity, demanded a willingness to lower barriers to foreign imports, even at the expense of domestic producers; and nothing so irritated him as the clamor of businessmen for protection. “Daily I am impressed by the short-sightedness bordering upon tragic stupidity of many who fancy themselves to be the greatest believers in and supporters of capitalism . . . but who blindly support measures and conditions that cannot fail in the long run to destroy any free economic system,” he angrily wrote in his diary. Many businessmen, he complained, were “so concerned for their own particular immediate market and prosperity that they utterly fail to see that the United States cannot continue to live in a world where it must, for the disposal of its products, export vast portions of its industrial and agricultural products unless it also imports a sufficiently great amount of foreign products to allow countries to pay for the surpluses they receive from us.”⁷¹ Similarly, Americans had to be willing to bear the costs of collective security, if for no other reason than to avoid what he believed would be the far greater costs of military and economic isolation. To his friend Hazlett he wrote that “we must pursue a broad and intelligent program of loans, trade, technical assistance and, under current conditions, mutual guarantees of security. We must stop talking about ‘give aways.’ We must understand that our foreign expenditures are investments in America's future.” He gave what he called a “simple example: No other nation is exhausting its irreplaceable resources so rapidly as is ours. Unless we are careful to build up and maintain a great group of international friends ready to trade with us, where do we hope to get all the

⁷⁰ Eisenhower to C. D. Jackson, April 30, 1957, DDEL, EPP, DDE Diaries series, box 13; Eisenhower to Frank Altschul, October 25, 1957, *ibid.*, box 16; and *Eisenhower Diaries* (February 9, July 2, 1953), 228–30, 242–45.

⁷¹ *Eisenhower Diaries* (July 2, 24, 1953), 242–49.

materials that we will one day need as our rate of consumption continues and accelerates." He bluntly told a group of prominent businessmen at a White House dinner that "we cannot have prosperity without security and we must have friends with whom to trade."⁷²

Eisenhower shared the conservative, anticommunist premises that animated both earlier and subsequent administrations, and he could act with ruthless efficiency when he believed that risks were limited and important national interests at stake. In Iran, where he had directed the overthrow of Muhammad Mossadegh and the return of young Shah Muhammad Reza Pahlavi, he believed that "we were in imminent danger of losing Iran, and sixty percent of the known oil reserves of the world," and he boasted privately that through his actions "that threat had been largely, if not totally, removed." In Guatemala, he ordered a highly secret CIA operation that overturned the moderately leftist government of Jacobo Arbenz Guzman and replaced it with the reactionary, but pro-American, dictatorship of Carlos Castillo Armas. In the Far East he was even willing to threaten nuclear war, especially against the Chinese, who had no capacity to retaliate in kind.⁷³ Yet for the most part his conduct of foreign affairs was distinguished by restraint, especially when the risks seemed high and the dangers of miscalculation great—he understood that the refusal to act was often the wisest course of action. As Robert Divine recently concluded, "Almost all of Eisenhower's foreign policy achievements were negative in nature. He ended the Korean war, he refused to intervene militarily in Indochina, he refrained from involving the United States in the Suez crisis, he avoided war with China over Quemoy and Matsu, he resisted the temptation to force a showdown over Berlin, he stopped exploding nuclear weapons in the atmosphere."⁷⁴

Yet Eisenhower could never quite transcend the logic of his premises, as his response to revolutionary nationalism clearly revealed. Like many sophisticated conservatives, he opposed traditional European colonialism as costly, impractical, and ultimately self-defeating. From his experience in the Philippines, moreover, he knew firsthand of "the intensity and force of the spirit of nationalism that is gripping all peoples of the world today." He believed, as he wrote George Humphrey, that the "*protection of our own interests and our own system demands . . . that we . . . understand that the spirit of nationalism, coupled with a deep hunger for some betterment in physical conditions and living standards, creates a critical situation in the under-developed areas of the world.*"⁷⁵ He had at

⁷² Eisenhower to Everett Hazlett, August 3, 1956, DDEL, EPP, DDE Diaries series, box 8; and Harlow, Memorandum for the Record, January 30, 1958, *ibid.*, box 18. For a critical examination of economic diplomacy under Eisenhower, see Burton I. Kaufman, *Trade and Aid: Eisenhower's Foreign Economic Policy, 1953–1961* (Baltimore, 1982).

⁷³ Eisenhower to Edgar Eisenhower, November 8, 1954, DDEL, EPP, DDE Diaries series, box 5, On Iran, especially see Stephen E. Ambrose, *At the Heart of the Matter: Eisenhower and the Espionage Establishment* (Garden City, N.Y., 1981), 189–214. On Guatemala, see Richard H. Immerman, *The CIA and Guatemala: The Foreign Policy of Intervention* (Austin, Texas, 1982); and Blanche Wiesen Cook, *The Declassified Eisenhower: A Divided Legacy of Peace and Political Warfare* (Garden City, N.Y., 1981), 217–92. On Asia, especially see Robert A. Divine, *Eisenhower and the Cold War* (New York, 1981), 28–70.

⁷⁴ Divine, *Eisenhower and the Cold War*, 154.

⁷⁵ Eisenhower to George Humphrey, March 1957, DDEL, EPP, Administration series, box 23; Eisenhower-

first believed that the free flow of goods and capital would in itself sustain economic development and that cooperation among nations and a friendly door to private investment would promote growth throughout the world. He later came to believe that enlightened self-interest required that the operation of the international market be supplemented by public capital. He expected, however, that new nations would follow the American model of capitalist growth.⁷⁶ Self-determination did not include the right to choose a radical road to development. Nor could he ever disentangle his response to social revolutions from his reaction to the foreign and military policies of the Soviet Union and China, as in the case of Indochina. If Eisenhower displayed restraint by refusing to intervene on behalf of the French in Indochina, it was a restraint produced more by France's refusal to grant its colonies full independence and permit the United States a decisive role in the military conduct of the war than by any particular reluctance on Eisenhower's part to employ force against social revolutions. The president wrote Hazlett in October 1954 that he had been unable to obtain "the conditions under which I felt the United States could properly intervene to protect its own interests." Eisenhower was determined, moreover, to draw the line in Southeast Asia—"we have got to keep the Pacific an American lake," he told his advisors.⁷⁷ Following the French collapse he committed the United States to the support of a client state south of the seventeenth parallel and to the undermining of the agreements reached at Geneva in 1954. These actions, as much as any, led to the expanded American involvement in Vietnam in the decade that followed.

Nor, finally, could Eisenhower escape the costly and destructive momentum of the warfare state. He believed that the Soviet challenge to the United States was indefinite, not immediate, and that it posed an economic and political threat as well as a military one. He was convinced that high levels of defense spending, such as those that had accompanied World War II and Korea, could not be indefinitely sustained without producing economic disorder and a resort to pervasive state intervention. As president he sought to reduce the level of America's defense effort, the so-called New Look, and was willing to permit by the end of the decade a relative increase in Soviet power. He embraced the concept of deterrent sufficiency rather than superiority—"why have more when we have as much destructive power as we do now," he asked Congressional leaders. Although he understood the problems posed by limited wars—"the enemy's political and military nibbling," he called it in a letter to Winston Churchill—he remained reluctant to "deploy and tie down our forces around the Soviet periphery in small wars."⁷⁸ His efforts to hold

er to Paul G. Hoffman, June 23, 1958, HSTL, Hoffman Papers, box 28; and Legislative Leadership meeting, July 2, 1957, DDEL, EPP, Legislative series, box 2.

⁷⁶ See, for example, Eisenhower's speeches to the Brazilian Congress, to a luncheon in São Paulo, Brazil, and to the Argentinian and Chilean congresses, during his 1960 trip to Latin America; *Eisenhower Public Papers*, 8: 216–20, 225–37, 256–61. "Investment capital is limited," he told the Chileans. "Competition for it is keen . . . and it will flow only to those areas where it is actively sought, welcomed, and treated fairly." *Ibid.*, 260.

⁷⁷ Eisenhower to Everett Hazlett, October 23, 1954, DDEL, EPP, Name series, box 18; and Conference in the President's Office, June 2, 1954, DDEL, EPP, Administration series, box 11.

⁷⁸ On defense policies, see Eisenhower to Charles E. Wilson, January 5, 1955, DDEL, EPP, Administration series, box 45; Eisenhower to Churchill, January 25, 1955, DDEL, EPP, DDE Diaries series, box 5; and

down military spending drew sharp criticism from within the armed services and Congress, however, and during his second administration the struggle to maintain what he believed was an appropriate balance between the nation's military and economic requirements consumed much of his energy.

The battle to hold down defense spending also forced Eisenhower to confront some of the dilemmas raised by his reliance on partnership and professionalism. He had deliberately chosen an industrialist to head the defense department—"We have earlier tried two investment bankers, a lawyer and a soldier," he observed—in the hope of imposing discipline and order on the services and strengthening cooperative relations with business. Many of his other defense appointments were also drawn from industry and finance, and the industrial advisory system begun during earlier administrations was expanded and strengthened. Similarly, Eisenhower hoped to recruit disinterested and expert military advisors, especially for the Joint Chiefs of Staff, who could rise above the petty loyalties of the services and, together with the civilian leadership, help promote broad national goals. He was, in all of this, disappointed. Wilson and the other businessmen who staffed defense failed to impose order, and, although Eisenhower succeeded in reorganizing the department in 1958, he increasingly came to believe that its problems were systemic. Service rivalry continued almost unabated, with each branch seeming to believe that it was "exclusively responsible for the defense of the United States." The Joint Chiefs of Staff failed to provide "disinterested, competent advice," and instead in many instances they became special pleaders for their services. Most importantly, Eisenhower came to believe that defense contractors themselves were exercising far too much influence over military budgets, and he expressed a keen interest in John J. McCloy's observation that "the inter-service game extends right down through the corporations, depending upon which branch their contracts flow from and it even goes into the academic institutions depending from where their research grants flow." In his farewell address Eisenhower noted the "conjunction of an immense military establishment and a large arms industry" and warned against "the acquisition of unwarranted influence . . . by the military industrial complex."⁷⁹

THE EISENHOWER PRESIDENCY was thus shaped by the self-conscious quest for a corporate commonwealth in which the contradictions of modern capitalism would

Eisenhower to Frank Altschul, October 25, 1957. On the concept of sufficiency, see Eisenhower's remarks, Legislative Leadership meeting, March 1, 1955, DDEL, EPP, Legislative series, box 1; Legislative Leadership meetings, February 28, 1956, June 24, 1958, December 15, 1958, *ibid.*, box 2. On limited wars, see Eisenhower to Churchill, January 25, 1955; Andrew J. Goodpaster, Memorandum for the Record, May 23, 1956, DDEL, EPP, DDE Diaries series, box 8; and Cabinet meeting, May 23, 1958, DDEL, EPP, Cabinet series, box 11. Also see Douglas Kinnard, *President Eisenhower and Strategy Management* (Lexington, Ky., 1977).

⁷⁹ Eisenhower to Alfred M. Gruenther, November 26, 1952, DDEL, EPP, Administration series, box 16; Eisenhower to McCloy, May 10, 1958, DDEL, EPP, DDE Diaries series, box 20; Andrew J. Goodpaster, Memorandum for the Record, May 18, 1956, *ibid.*, box 8; Eisenhower to Everett Hazlett, August 20, 1956, *ibid.*; Legislative Leadership meeting, June 2, 1959, DDEL, EPP, Legislative series, box 3; Notes on Pre-Press Conference Briefing, June 3, 1959, DDEL, EPP, Ann Whitman Diary series, box 10; and *Eisenhower Public Papers*, 8: 1038.

be resolved through cooperation, self-restraint, discipline, and disinterested public service. The power of the state would be carefully limited, budgets prudently managed, cooperative arrangements forged between business and government, and conflicts defused through skillful governance and public relations. Enlightened diplomacy would similarly resolve potential conflicts among both developed and developing states and ensure a stable and harmonious world order. Only by understanding the centrality of this quest can we begin to grasp the inner coherence of the Eisenhower presidency and, more importantly, its relationship to the twentieth-century search for a new political economy. Indeed, the struggle to define the character of that new system has been the most important issue in modern American politics, from Populism and Progressivism through Hoover and the New Deal to the contemporary debate over “reindustrialization” and the proper relationship between government and economic life. In understanding Eisenhower we begin to understand our past, our present, and—at least in part—the alternatives before us. It is precisely because of this resonance, however, that we must be careful to avoid facile and misleading analogies and to label any conclusions provisional.

Eisenhower’s quest for a new order was, on one level at least, an enormous success. The years of his presidency were among the most prosperous, peaceful, and politically tranquil in this century, and he left office one of the most popular chief executives in American history. But this was, as he himself would have been quick to note, a calculus of only short-term results. He had succeeded in slowing the growth of the federal state and, as in the case of the highway program, insulating its operations from popular politics. He had also succeeded in expanding cooperative arrangements between government and business and in accelerating the interpenetration of public and private sectors. In all of this, of course, his presidency served to rationalize the efforts of American business to refashion the New Deal state. But he did not succeed in securing that concert of private interests that would insure stable and orderly growth and on which his vision of a corporate commonwealth depended. Indeed, before his presidency had ended there was already widespread evidence of disorder and dysfunction: business leaders had repeatedly failed to exercise the restraint and self-discipline that such a system demanded, the problems of agriculture had proven politically irresolvable, and employers and labor unions were increasingly “tending to settle their differences without regard to the impact on the economy.” Nor did he succeed in surmounting the tensions of class, race, and sex that surged like powerful undercurrents just beneath the surface of American culture. The tendency, encouraged during his presidency, to substitute private consumption for public politics laid a heavy—if then still invisible—tax on limited resources, increased political alienation, and undercut his own emphasis on sacrifice and discipline. Revolutions throughout the Third World created growing international tension and heightened the conflict between his tactical emphasis on the limits of American power and the globalism to which he and other American leaders continued to subscribe. Critics, including some former supporters, now began to accuse the president of lack of leadership, demanding not only a more activist foreign policy but also a level of military spending that he clearly feared

would produce disastrous consequences. By 1958 he was wondering plaintively “whether immediate greed would ever surrender to the long-term good of the whole world.” His attempt to fashion a corporate commonwealth foundered, finally, on the problem of succession and on the inability of the Republican party to generate a disinterested leadership capable of sustaining his vision. His deep ambivalence over Nixon turned on precisely this point—“it is terrible,” he pointedly told Ann Whitman, “when people get politically ambitious.”⁸⁰

Within a decade the fragile consensus of the 1950s shattered on the hard realities of war and revolution, of class and racial conflict, of repression and indulgence. Such a failure was probably inevitable; for despite his pragmatism and lucid intelligence Eisenhower was at heart a visionary. Alarmed by the self-interested destructiveness of contemporary economic life, he had fashioned a deeply conservative image of a good society in which conflict would yield to cooperation, greed to discipline, coercion to self-government. Such a vision was no match for the vast and powerful forces of modern America.

⁸⁰ Cabinet meeting, December 14, 1956, DDEL, EPP, Cabinet series, box 6; Legislative Leadership meeting, May 13, 1958, DDEL, EPP, Legislative series, box 2; and Whitman Diary, June 11, 1959, DDEL, EPP, Ann Whitman Diary series, box 10.

AHR Forum

Comparative History in Theory and Practice: A Discussion

IN OCTOBER AND DECEMBER 1980, the *Review* published two successive issues devoted to comparative history, its theory and practice (*AHR*, 85 [1980]: 753–857, 1055–1166). Most of the articles in these issues were spontaneous submissions—received “over the transom,” in editors’ jargon. Although two manuscripts were submitted on our invitation, none was commissioned by the editors. We believed the essays to be representative of current research in comparative history and published them in the hope of demonstrating that history, which often seems to have fallen victim to the fragmentation typical of modern scholarship as a whole, can still be a unified discipline.

The editors are pleased to report that the issues aroused a wide resonance among our readers. The comments they sent us and the replies that came from the authors concerned were long, thoughtful, and often witty (something rare in these pages). Because of their intrinsic interest, we have elected to print them in the *Forum* rather than in the Communications section. The sequence of letters below comprises all of the comments received by July 1, 1981, and all replies on hand as of October 1, 1981. The articles we published in the two comparative history issues are as follows:

- Alexander, Thomas, *et al.*, “*AHR Forum*—Antebellum North and South in Comparative Perspective: A Discussion,” 1150–66;
Bowman, Shearer Davis, “Antebellum Planters and *Vormärz* Junkers in Comparative Perspective,” 779–808;
Earle, Carville, and Ronald Hoffman, “The Foundation of the Modern Economy: Agriculture and the Costs of Labor in the United States and England, 1800–61,” 1055–94;
Grew, Raymond, “The Case for Comparing Histories,” 763–78;
Hill, Boyd H., Jr., and Alette Olin Hill, “*AHR Forum*—Marc Bloch and Comparative History,” 828–57;
Kolchin, Peter, “In Defense of Servitude: American Proslavery and Russian Proserfdom Arguments, 1760–1860,” 809–27;
McDonald, Forrest, and Grady McWhiney, “The South from Self-Sufficiency to Peonage: An Interpretation,” 1095–1118; and

Pessen, Edward, "How Different from Each Other Were the Antebellum North and South?" 1119–49.



IT IS A PLEASURE TO PRAISE the articles by the five contributors—Raymond Grew, Shearer Davis Bowman, Peter Kolchin, and Boyd and Alette Hill—to “Comparative History in Theory and Practice, I.” The following observation is intended as a tribute to their stimulating essays. I believe that Professor Grew would readily admit that often the kinds of conclusions at which one arrives in comparative history are dependent upon what one chooses to compare.

Professor Kolchin has persuasively argued that Russian serfowners offered less resistance to emancipation than antebellum planters because only their economic interests, not their entire way of life, were threatened; yet he went on to cite the well-known generalization that the Russian *pomeshchiki* were “dependent, timid, and socially marginal” (p. 826). Dr. Bowman has made a similar comparison, and it appears that the Junkers made a decision identical to that of the Russian serfowners: “The Junkers could afford to sacrifice some of the traditional character of the *Rittergut* as a private law state, since, unlike planters, they were privileged members of a larger hierarchical and authoritarian political order. Political flexibility, even more than the weakness and divisiveness of their foes, gave the Junkers a durability that history denied to the planters” (p. 808).

The Prussian Junkers have never been accused of being “dependent, timid, and socially marginal,” yet they pursued the same strategy as the Russian *pomeshchiki*. Does not this comparison between Junkers and serfowners suggest at least one different conclusion than the comparison between planters and serfowners—namely, that perhaps the Russian serfowners, as “privileged members of a larger hierarchical and authoritarian political order” capable of “political flexibility,” were far closer to the Prussian Junkers in social and political influence than historians have supposed? It would certainly be interesting to have Professor Kolchin’s comment on this possibility.

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PERHAPS PETER KOLCHIN might wish to consider the objection to his labeling of Southern defenders of slavery as “conservative” in light of a larger objection to the theoretical suppositions that sustain comparisons of the type upon which his study rests. Is there not a derogation of reason and thought—not only of so-called conservative thought—in the supposition, essentially historicist and Marxist, that

underserves the comparison of ideas (in Kolchin's terminology, "ideologies") held by Southern slaveholders and Russian serfholders?

Professor Kolchin has suggested that it is "ahistorical . . . [to think] that people living in the past must share our values" (p. 810). But, then, what possible interest can we have in knowing what those values were beyond the merest antiquarian curiosity? Indeed, how can we even know what those past values were? Suppositions of the type Kolchin has proposed permit us to have the best (some would say the worst) of a thesis that gives to man only historical knowledge. Thus, we shall be granted no universally valid claims except, as it were, historical ones: we shall be granted knowledge of limited claims, whose limits are possessed of a universal character in the degree that they are somehow scientific (additive or connected)—for example, comparisons—and this knowledge we shall have liberty to call universal—that is, not historical, contingent, or discrete. But this, logic aside, assumes laws of human evolution (Marxism? folk minds?)—that is, knowledge of a type that is not provided in history and certainly not by the views of Kolchin, who considers it "ahistorical" to suppose that values are shared over time.

These theoretical aspects have direct bearing upon historical studies, in particular upon the realm of the empirical accessible to any historian. In other words, these theoretical aspects bear upon accuracy in historical studies. There is an important case of values shared over time, which should be of special interest to Professor Kolchin's suppositions and to his designation of the Southern argument in support of slavery as conservative.

Harry Jaffa has observed that there exists a "tacit alliance between the epigones of Karl Marx and those of John C. Calhoun which dominates the American intellectual climate today" (Jaffa, "Inventing American History: Wills on Jefferson") *Conservative Historian's Forum* [February 1982]). This continuity of values is the less surprising if we recall the similarity of the proslavery attack on the North and the attack of Marx and Engels on capitalism in the same period. Undoubtedly, the proslavery defense was not conservative in the pejorative sense that attaches to that term in the *American Historical Review* and in Professor Kolchin's essay. The socialist, thus presumably progressive, critique of Northern and all other "unconscionable freedom" (Marx's phrase) was hotly argued in the South, and not only by George Fitzhugh, a fact Kolchin has stressed. And what Fitzhugh called the "hairbreadth" of difference separating Northern and European socialists from Southern ones—namely, the difference between an overseer and a phalanx (also requiring an overseer, as a matter of fact)—can hardly be said to constitute a difference in kind and may scarcely be a difference in degree. "We assert a theory bluntly and plainly," complained Fitzhugh, ". . . and the world is ready to exclaim, 'Oh, what shocking heresy.' Mr. [Horace] Greeley, for twenty years, maintains the same theory, in different language, and elicits the admiration and gratitude of the world." But the matter is not one of labels.

The supposition that we cannot share the values of the past or the opinion, formalized in the work of historians J. G. A. Pocock and Quentin Skinner, that there are no perennial questions, entails a view of thought such as Professor Kolchin's. The dangers of this view, as suggested briefly here, are above all

historical misperceptions or, to put it differently, the dangers of this view involve the misconstrual of the historical as the political, actually the derivation of the political from the historical. Fitzhugh's political theory was in the main a defense of socialism and of Abolitionism to the degree (and it was not a small degree) that these doctrines were critical of free society. That socialists and Abolitionists were also unfriendly to slavery, Fitzhugh counted an aberration, a logical and sentimental oversight.

Whether Fitzhugh's political theory was coherent is not the issue here. Rather the question is whether Professor Kolchin has rendered it accurately. Conservatism is, of course, liberalism in the nineteenth century and either reaction or radicalism in the twentieth. This is not because times and places are constitutive of thought but because history is not political theory. Any historical comparison rooted in an understanding of theory that mistakes the historical for the political is unlikely to tell us anything of interest about theory or value even as it is likely to misconceive theoretical distinctions. Misconceptions of this type tell us what we might perhaps have guessed, and what at all events we can have no interest in knowing—especially if it is “ahistorical” to suppose that “values” transcend the historical. This is a practical result of metaphysical and ethical observations of the type that stand at the head of Kolchin's essay: for example, his view that “a generation believing in the axiomatic virtues of freedom and equality—however they may be defined—finds little to celebrate in men who defend . . . inequality and the restriction of . . . freedom” (p. 809). Surely this is a most astonishing proposition, as if we had forgotten the words of Kafka's lecturer, who said, grinning as it were from a future that still eludes us, “In passing: May I say that all too often men are betrayed by the word freedom?”

ROBERT J. LOEWENBERG
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I AM AN ADMIRER of Peter Kolchin's pioneering ventures into comparative history both in the *American Historical Review* and in the *Journal of Social History* (1978). His discussions of Russian serfdom and American slavery provide new insights into both subjects and compel specialists to reconsider many of their preconceptions. Yet, if Kolchin's purpose is indeed to bring us closer to the thinking of serfholders and slaveowners (pp. 809–10), then he might have carried his war on ethnocentrism a little further.

In his conclusion (p. 823), Professor Kolchin suggested four general differences between Russian and American upholders of servitude. They take the form of four elements in American society that were absent in Russia, and these elements, Kolchin stated, explain why American slaveholders defended their privileges more vigorously than Russian serfholders did. The four elements are (1) racial distinction between owners and owned, (2) democratic government, (3) free press, and (4) confinement of servitude to a specific section of the country.

I have no scholarly quarrel with this formulation. May I suggest, however, that, since Professor Kolchin's work is directed to an American audience, he might have

sharpened the impact of his comparison by formulating the differences between the two societies in terms of features that were present in Russia and lacking in the United States? Had Kolchin done this, had he undertaken to explain the weakness of the Russian serfholders' defense of their privileges by referring to features of *Russian* society, he could have reduced his four differences between Russia and the United States to one: autocracy. If Russian society passed more smoothly out of serfdom and then launched itself into a gradual rise in the former serfs' social status, economic welfare, and educational level, this was primarily because Russia's "elite" were organized in military fashion under an autocrat. From 1905 to 1917 progress toward equality was hindered to some extent by those two formidable weapons of privilege, the vote-gathering party and the profit-dependent newspaper, but it was not blocked entirely. This simple statement could replace Kolchin's second and third points quite well. (Obviously, I am extending the word "privilege" to include all pious-liberal and sanctimonious-academic varieties.)

Professor Kolchin's first and fourth points—the racist and sectional nature of American slavery—lose some of their significance when seen from a Russian perspective. There were sections in the empire where a deep cultural gap between owner and owned could be found, a gap that may have been at least as significant as skin color. Contrary to Kolchin's general explanations, however, these were the areas where autocracy enjoyed its greatest success as an institutional basis for attacking the privileges of serfowners. Conversely, these were the areas where serfowners were *least* able to preserve their privileges.

The Baltic *gubernii* saw perhaps the most extreme cultural distinction between owners and owned. Nevertheless, despite ever-growing tensions between German lords on the one hand and Estonian and Latvian peasants on the other, this was the first area where autocracy was able to abolish serfdom. Moreover, the newly freed serfs experienced the most rapid economic and cultural advance of any native peasantry in the empire. In this case, then, confinement of the institution under attack to a single section and a sharp cultural gap between owners and owned seem to have strengthened autocracy's capacity for dissolving privilege.

Following Professor Kolchin's America-centered model, stronger autocracy should have been equivalent to a weaker nobility. For historians of the United States, explaining Alexander I's ability to force the Baltic Germans to free their serfs by seeking out some "weakness" or "lack" in Baltic German institutions is quite natural. Indeed, it is virtually inconceivable to U.S. historians that people now and then have something on their minds besides their "interests"; therefore, autocracy is by definition oppressive, and only weak or docile people accept its dictates. But the Baltic Germans were not weak and docile. They were more progressive, commercialized, and enlightened than any other group of serfholders in Russia. Certainly they were the best organized. Nevertheless, unlike their similarly commercialized and well-organized counterparts in the Southern United States, they yielded to pressure from the central government and readily gave up their serfs. In other words, the strength of Russia's autocratic government, especially its characteristic power to set aside the rights of citizens arbitrarily, did not always imply the backwardness of the people, the absence of citizens, or the weakness of local

institutions. Perhaps, then, autocracy in Russia signified (and signifies) something more than an absence of American institutions.

Then there were the Polish and pseudo-Polish *gubernii*. Here, conditions accompanying serf emancipation came closest to resembling those obtaining in the United States in the 1860s. Serf emancipation took place during a full-scale rebellion led by serfholders calling themselves champions of liberty. As in the American South, serfholders of one section rebelled against government policies directed against their “homeland” as well as themselves. Moreover, in the pseudo-Polish *gubernii* of Bielorrussia, Lithuania, and the right-bank Ukraine, there was even a cultural gap between owners and owned. Serfs were Orthodox, and the lords Roman Catholic. Thus, Polish behavior in 1863, unlike that of the Baltic Germans a few decades earlier, seems to fit Professor Kolchin’s general explanations about the strength of serfholder resistance.

But a Russian perspective casts a different light on the struggles of Polish serfholders and American slaveholders to preserve their “rights.” If emancipation in the former territory of the Polish Kingdom is contrasted with the process that occurred in the rest of European Russia, the first thing that strikes the observer is the more radical character Polish emancipation took *after* the lords’ rebellion was crushed. Newly freed serfs in the pseudo-Polish *gubernii* received far more generous allotments of land on far more generous terms than serfs did anywhere else. Even in Poland proper, where Napoleon had freed the peasants from serfdom half a century earlier, the semi-free peasants received yet further generous offerings of political and civil rights, together with more land. Why did Polish lords lose so much more after their rebellion than their Russian counterparts? The apparent answer is that they rebelled. Polish serfholders were more inclined to fight for their prerogatives, but that they fought and lost rendered them extremely vulnerable to autocratic power. Those Great Russian gentry and bureaucrats who had bitterly opposed emancipation of their own serfs before 1861 were generally delighted in 1863–64 to witness their tsar making his assault on Polish serfholders. So, Polish serfholders were comparable to slaveholders in the American South in their ability to launch a rebellion, but Polish privilege suffered a mortal blow *after* the rebellion—a blow that the U.S. government was incapable of landing on the conquered whites of the South. Why? Because the United States lacked autocracy. America had no institution for compelling privileged people to perform duties corresponding to their privileges. This difference between emancipation in Poland and black “liberation” in the American South may be more significant than the similarities Professor Kolchin pointed out.

One should not dismiss the United States as a land entirely lacking in autocracy. America did take a few steps toward black liberation in the 1860s, and this was largely because the federal government momentarily held the conquered South under an autocratic military regime. Of course, as we know, the party machine and the profit-dependent newspaper editor quickly re-established their grip on American life, thus stifling all sense of social duty in the privileged classes and dooming the black population to wait several decades before the shocks of two world wars at last blew open the way for their educational and economic advance. Even so,

Abraham Lincoln's halting flirtation with autocracy was not without its ultimate importance. Had Lincoln and the reconstructionists not acted arbitrarily, in something like Russian fashion, it is hard to imagine how slavery ever would have come to an end in the United States.

Such are a few of the remarks one might make about Professor Kolchin's study from a Russian perspective. Obviously, they are only interpretive, not substantive. They are only variations on a theme by Kolchin, and I want to thank him, not criticize him, for doing much to clear the murky, stultifying atmosphere of stale preconceptions in which U.S. historians ply their trade.

GEORGE YANEY

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I APPRECIATE George Yaney's comments on my article, "In Defense of Servitude," especially since they are, as he puts it, from a Russian perspective. Although we differ over some matters of interpretation, I find myself in general agreement with the thrust of his remarks, and I welcome the opportunity they provide me to clarify—both here and in the future—three major points.

I fully concur in Professor Yaney's judgment that the existence of autocracy played a critical role in limiting the Russian defense of serfdom. Indeed, although Yaney's formulation of this thesis is somewhat different from my own, the thesis itself is clearly implicit in my emphasis on the presence of democratic government and a free press in the United States and their absence in Russia. Here, Yaney's autocracy, present in Russia but absent in the United States, is simply the obverse of my democracy and free press, present in United States but absent in Russia. Where we differ is over Yaney's apparent contention that autocracy is the *sole* explanation for the weakness of probondage sentiment in Russia compared to that in the United States. As I suggested in my article, I believe the most fundamental cause of this difference lies in the contrast between the relative independence of American slaveowners and dependence of Russian serfowners. Autocracy and democracy were important causes of *pomeshchik* dependence and planter independence, but not the only important causes; equally important were relations between the masters and bondsmen in the two countries.

Professor Yaney is correct, of course, that there were portions of the Russian Empire—especially along its western periphery—where cultural and religious distinctions existed between owner and owned. Nevertheless, there can be little doubt that from a broad perspective Russian serfdom lacked the ethnic distinction between master and bondsman present in American slavery. Most Russian serfs shared a common national ancestry with their owners; virtually no American slaves did. Put most simply, American slavery, unlike Russian serfdom, was racially based, and it seems to me that Yaney underestimates the significance of this racial basis.

As Professor Yaney suggests, ethnic distinction between owner and owned did not in itself necessarily accentuate the defense of bondage, or make emancipation more difficult. His example of the Baltic provinces proves this, as does emancipa-

tion in most of Latin America, where a racially based slavery died more gracefully than it did in the United States. What was peculiar about the situation in the United States was not the racial basis of slavery—here clearly it was Russian serfdom that was unusual—but the peculiar nature of race relations and racial attitudes that it engendered, relations and attitudes that differed in important ways from those prevalent in other racially based slave societies. The question of race cannot be viewed in isolation any more than can that of autocracy: race—in conjunction with democracy, sectionalism, a free press, and particular demographic conditions—created a slaveowning class in the Southern United States that was unusually independent, and it was this independence, I am convinced, that is most crucial in explaining the unusual stubbornness with which Southern slaveowners clung to their “peculiar institution.”

I agree with Professor Yaney’s final point that where emancipation was precipitated by a conservative rebellion launched by the owners the resulting changes were likely to be more far-reaching than they were elsewhere. This was true not only in Poland but also in the U.S. South, where—unlike the vast majority of slave societies undergoing emancipation—owners were uncompensated for their sudden loss of property and a major (though at best only partially successful) effort was made to provide the freedmen with civil and political rights equal to those of their former masters. If their independence made Southern slaveowners more stubborn than most in resisting emancipation, their rebellion and military defeat rendered them less able to institute or shape the new order. Here, too, the Russian comparison is instructive.

Although I do not pretend to any expertise on the subject of Prussian Junkers, I also find plausible Charles Halperin’s suggestion that they resembled Russian serfowners “in social and political influence.” Both groups were “privileged members of a larger hierarchical and authoritarian political order”; furthermore, neither had to contend, as Southern slaveowners did, with political democracy, a free press, a sectionally based abolitionist attack, or the perceived threat of racial amalgamation. Here, once again, the unique position of the Southern planters is evident. At the same time, however, because Junkers were more independent than *pomeshchiki*, they were able (as Shearer Davis Bowman has shown) to mount a more serious ideological response to their “revolution from above” than Russian serfowners were to theirs.

I find Robert J. Loewenberg’s discourse more problematical. Quite simply, he and I disagree fundamentally about the nature of history. He evidently believes that all people, in all times and places, have shared the same God-given values; I do not. Readers can decide for themselves whose position leads to “historical misperceptions.”

PETER KOLCHIN

University of New Mexico



THE *American Historical Review* HAS SEEN FIT to publish a version of the English North–Celtic South theory of American cultural sectionalism that Forrest McDonald and Grady McWhiney have propounded several times elsewhere. Neither of the commentators in the accompanying *AHR Forum* dealt with that part of their article, although Thomas B. Alexander did observe that it will find challengers (pp. 1,152–53). It has.

To substantiate the dubious axiom that Lowland Scotland, Ulster, and even northern and western England have always had much the same Celtic culture as the Highlands, southern and western Ireland, Wales, and Cornwall, Professors McDonald and McWhiney have systematically misrepresented a number of contemporary travelers and recent historians. “As J. G. A. Pocock has put it,” they stated, “the Celtic peoples are ‘no more English than Britain is European’ ” (p. 1,109). Pocock’s own words, in the place cited (but making a quite different point), are, “Scotland is no more English than Britain is European” (Pocock, “British History: A Plea for a New Subject,” *New Zealand Journal of History*, 8 [1974]: 7, 10). Denys Hay’s article on the Anglo-Scottish border country and A. L. Rowse’s *The West in English History*, both also cited in note 31, do claim a distinctive character for each of those regions, but neither ever suggests that the distinction was “Celticness.” The people whom Thomas Pennant described in 1769 as “indolent to a high degree, unless roused,” he called (in the same sentence) “the native Highlanders,” not McDonald and McWhiney’s “the Scottish plain folk” (p. 1,110; and Thomas Pennant, *A Tour of Scotland, 1769* [4th edn., London, 1776], 213). In short, where their sources speak of Celts, they have substituted undifferentiated Scots; where their sources say Scots, West Countrymen, or Ulstermen, they have substituted Celts. A false premise is made its own proof.

Presumably in order to refute critics of their previous articles on the subject, Professors McDonald and McWhiney have now referred obliquely to one of their most stubbornly held notions: that Highlanders, like Southerners later, were great swineherders. (In fact, Highlanders were so averse to pigs and pork that they were sometimes called “the Jewish Scots”; James E. Handley, *Scottish Farming in the Eighteenth Century* [London, 1953], 73 n. 2.) Thus, McDonald and McWhiney paraphrased Estienne Perlin as speaking in 1558 of “the great abundance of cattle,” and they advise us that the latter was “a term that comprehends sheep, horses, and swine as well as bovine animals” (p. 1,110). It did once mean livestock in general—and, before that, chattels or capital of any sort—but all of that is pointless here, since Perlin’s words were “asses [that is, “assez”] de veaux et vaches.” Even the English translation cited by McDonald and McWhiney makes it “plenty of cows and calves” (Perlin, *Description des Royaumes d’Angleterre et d’Ecosse* [1558; London, 1775], 33; and P. Hume Brown, ed., *Early Travellers in Scotland* [Edinburgh, 1891], 74). Likewise, Bishop Leslie, from whose description McDonald and McWhiney took a brief excerpt in a secondary work, did not in fact refer to “every manner of livestock” roaming about on the Scottish open range, as they asserted (p. 1,110), but to Highland “hart and hyne, cattel, and wyld beistes,” only one of which was livestock. Furthermore, they omitted from the passage quoted Leslie’s specific reference to “ky”—the plural of “cow.” Elsewhere he used “ky” and “cattel”

interchangeably. Had Leslie's further observation—that beef was salted in Scotland “as swyne fleshe is vset in vthir cuntries, of quhilke our cuntrie peple hes lytle plesure”—been consulted, the Celtic theory of Southern swineherding and pork-eating could have been laid to rest (P. Hume Brown, ed., *Scotland before 1700 from Contemporary Documents* [Edinburgh, 1893], 132, 133, 138).

Since they evidently have not visited the English West Country, Professors McDonald and McWhiney may be excused for accepting the notion of an antiquarian of 1899 that the Virginia accent, even at that late date, was distinctively West Country (p. 1,108 n. 31). But what can be said for their supposition that Devon, Somerset, Dorset, and Wiltshire acquired the peculiarity of their dialect of English from Cornwall rather than that the Cornish learned it from their West Country neighbors? Here again, the desired conclusion dictates the premise from which it is ostensibly drawn: that the whole of the ancient territory of the West Saxons, as a “Celtic frontier,” must itself have remained more Celtic than English.

Even for Southern speech, the ear of Professors McDonald and McWhiney seems tone deaf. In asserting that the “rural Virginia ‘oot’ for ‘out’ is clearly of Scottish origin,” they have overlooked the observation of Cleanth Brooks, Jr., in a work that they cited (imprecisely) on such matters, that Virginians actually pronounce “out” not as “oot” but rather as “eut”—that is, with the diphthong [əʊ], which is no closer to the Scottish pure vowel [u:] than the diphthong [aʊ] spoken elsewhere (Brooks, *The Relation of the Alabama-Georgia Dialect to the Provincial Dialects of Great Britain* [Baton Rouge, 1955], 73, 86–87). Even this simple bit of data is mistaken, and so too the ethnic inference from it.

Professors McDonald and McWhiney attempted to disarm criticism by explaining that “Celtic” is simply the most “catchy” term available and that they do not really mean that all of the peoples whom they have designated Celts were ever exactly alike (p. 1,108 n. 31). Unfortunately, their interpretation of Southern culture, including their quantitative analysis of surnames in other articles, has been deduced from precisely the premise that all of the British Isles except southern and eastern England shared fairly uniform, perdurable Celtic traits. Whatever common character one may detect among Welshmen and Cornishmen, and Irish and Scottish Gaels, after their separation of one or two thousand years, they have not in recent times constituted a single non-English ethnic type, certainly not one that can be called “Celtic.” As for Highlanders and Lowlanders, or Irish and Scotch-Irish, they cannot be set off against Englishmen in the way that, as McDonald and McWhiney put it, “Italians and Swedes differ from . . . Libyans and Turks” (p. 1,108 n. 31). Lowlanders and Ulster Scots, after all, have spoken variants of English throughout history; as Perlin observed, “Les Escossois parlent comme les Anglois ou pour le moins n’y a pas grande difference et distance, telle qu’il y a de la langue des Normans a celle des Picards” (*Description des Royaulmes d’Angleterre et d’Ecosse*, 31). To the Gaelic-speaking Highlander, however, the Lowlander was a *Sasunnach* (“Saxon”) and his language *Beurla* (literally, “jargon”). As for Libyans and Turks, one might likewise say that to the Irish Gael the Lowland Scottish intruder into Ulster was as great a Turk as the Englishman who sent him.

There are other oddities here, such as the sheer deduction from a description of

1824 that the Irish did not commonly plant crops before the potato (and that not until the 1780s). But why harp on what anyone with the least interest in such matters will recognize as a hopeless muddle? Unfortunately, three of our leading journals have now placed their editorial imprimatur on it. Even newspaper feature writers have been more skeptical (*Houston Chronicle*, November 30, 1980, sec. 2, p. 10). But, then, perhaps the display of proverbial "Celtic" indolence in research and impetuosity in publication by two Southern historians is itself a demonstration of their thesis.

ROWLAND BERTHOFF
Washington University

AN OBSERVATION OR TWO to throw into the bubbling cauldron of debate on some of the theses advanced by Forrest McDonald and Grady McWhiney in their provocative (and provoking) essay on "The South from Self-Sufficiency to Peonage."

It appears that Professors McDonald and McWhiney have placed the beginning of British settlement of the South fully a century and more later than it occurred. The pattern of settlement on dispersed farms and plantations, which they identified as Celtic in type *and* origin, was in fact well established long before the floodtide of Celtic settlers had started to pour down over the Appalachian trail. Carville Earle has recently argued that some early urban-born English settlers, against the advice and wishes of their leaders, began to desert tidewater towns and disperse, for reasons of health, into the wilderness to establish remote farms and plantations (Earle, "Environment, Disease, and Mortality in Early Virginia," in Thad W. Tate and David L. Ammerman, eds., *The Chesapeake in the Seventeenth Century* [Chapel Hill, 1979], 95–125). The first English settlers in South Carolina dispersed even more quickly—indeed, almost from the first days of settlement. Here ambition for baronial estates embodied in John Locke's Fundamental Constitution for the Colony may have been a principal motive.

Reinforcing this motive very soon were economic opportunities. The large pine forests and numerous navigable waterways invited dispersed settlement for those who decided to deal in naval stores, and, as H. Roy Merrens has observed, coastal savannas similarly invited a large livestock-raising industry long before the arrival in significant numbers of Celtic cowboys (Merrens, ed., *The Colonial South Carolina Scene* [Columbia, S.C., 1977], 5–6, 110–21).

By the time the Huguenots settled along the Santee River in the colony's second generation, the Carolina plantation and farm economy and society were already in the process of coalescing; and, by the time Swiss immigrants settled on farms along the Savannah River and German immigrants on farms in what is still called the Dutch Fork country a generation later, the economy and society had coalesced. The new settlers, despite retention (particularly in the case of the Germans) of ethnic peculiarities, found themselves adapting to this new world and lifestyle fully a generation *before* the Scots and Scotch-Irish began to surge into the colony's piedmont.

And no wonder. The opportunities and realities in the Carolina backcountry

were very different from those, for instance, in the Palatinate, from whence the Germans had come.

DAVID MOLTKE-HANSEN

South Carolina Historical Society

WE WELCOME CRITICISMS of the sort offered by David Moltke-Hansen, even when they are unsound; for they force us to dig through and think through matters that we might otherwise have overlooked. Having dug and thought, we respond. What Carville Earle said in a stimulating essay we had not previously read is rather different from what Mr. Moltke-Hansen asserts that he said: Earle pointed out that Captain John Smith wisely tried to force the early Virginians, unsuccessfully and over their strong protests, to disperse in the interest of health, and that he was soon fired; “with him went the schemes of seminomadism and summer dispersal” (“Environment, Disease, and Mortality,” 106–08). The settlers, following their accustomed English ways of living in nucleated villages, “once again clustered at Jamestown, and death hung heavy” over them. Subsequent efforts by their leaders to enforce dispersion met with mixed results. Not until the 1640s and 1650s did Virginia settlers voluntarily “desert tidewater towns and disperse . . . into the wilderness”—and, as Frank Managhan showed in his doctoral dissertation (University of Virginia, 1946), it was at that time that the great wave of Celtic, west country, and north country immigrants started to arrive. Managhan, by the way, showed that of the 7,359 references to persons by name in seventeenth-century Virginia documents, no fewer than 6,647 of those persons originated in the Celtic fringe. “A century and more” before that would have been in the 1530s or thereabouts. We therefore think that we place our beginnings more or less accurately.

As for the first settlers in South Carolina, they did take up dispersed open-range herding quite early on, during the late seventeenth century—long before the arrival of the great waves of Scotch-Irish. But two demurrers must be entered. First (as the transcripts of the Shaftesbury Papers in Charleston make clear), the three original ships that took immigrants to South Carolina in 1670 stopped at Kinsale, Ireland, and took on a goodly number of passengers. They took on more in Barbados, and a large proportion of the other early South Carolinians came from Barbados—which was overpopulated, Cromwell having sent thousands of Irishmen there in the 1650s. Thus from the outset, a sizable fraction, possibly a majority, of the original settlers were Irish. Second, Peter Wood has pointed out in his admirable work, *Black Majority* (1978), that the open-range system unnerved the English settlers and that its success mightily upset the proprietors under the Fundamental Constitution. Wood argues that the people who instituted the system were Negro slaves who had practiced open-range husbandry in Africa. In any event, herds multiplied rapidly in South Carolina, but they were wiped out in the Yamasee War of 1715. Forthwith, the colony imported five or six hundred Ulstermen (Merrens, *Colonial South Carolina*, 58; and R. J. Dickson, *Ulster Emigration* [Belfast, 1966], 24), who immediately constituted nearly a tenth of South Carolina’s miniscule white population.

Now notice the timing. The document in Merrens's collection to which Mr. Moltke-Hansen refers is dated 1733–34. In 1731 South Carolina offered a bounty to Scotch-Irish immigrants, and during the next six or seven years several thousand Ulstermen availed themselves of the opportunity and made the move (Dickson, *Ulster Emigration*, 49–52). It was during those same 1730s that the livestock industry was re-established in South Carolina, along with that of the rice plantations. We Micks may not be good with figures, but we reckon that almost anyone would find it difficult to understand how events before and during the 1730s could have taken place “fully a generation” after events of the 1730s.

We greet Professor Berthoff in a somewhat less cordial spirit. Indeed, lesser men than we, upon being hounded by such an unrelenting critic as he, might long since have given up the game. Mr. Berthoff, whose wife is Scottish, regards as impalatable any scheme of things that treats the Scots and the lowly Irish as being of common stock, the evidence of history notwithstanding. Accordingly, he has fired off salvos of protest to a number of journals that have published our articles (he missed *History Today*, *Newsweek*, and sundry other minor rags). He casts his fulminations in heavily documented semitruths that are difficult to rebut, short of writing considerably more than any reasonable person could want to read. Sometimes we have ignored him, as we did in regard to his incomprehensible letter to the *Journal of Southern History*; sometimes, as at the Southern Historical Association meeting in Atlanta in 1980, we have patiently picked his arguments to pieces, reducing him to admitting in public, “I am incoherent.”

We shall, however, in the spirit of scholars and gentlemen, essay a reply. One: As we stated, we are aware of differences among people whose cultures were originally Celtic, and we use the term “Celtic” with both apologies and hedges. We are quite aware that Lowland Scots, for example, had been somewhat Anglicized for several hundred years before the eighteenth century, the time of heaviest migration to America. We share David Hume's belief that Lowlanders, culturally as well as genetically, were probably about a third each Celtic, Scandinavian, and Anglo-Saxon. BUT: Even in Hume's time, one could wander a scant fifty miles from Edinburgh and find people who had never seen a wheel and discover droves of hogs wandering the woods. Even in the 1770s, Boswell could record that Sir Allan Maclean had life-and-death power over the members of his clan. It can be demonstrated as well (and we shall do so in our forthcoming book) that the leading figures of the Scottish Enlightenment, like the seventeenth-century Stuart kings of England, thought and conceived of society and government in patterns that were characteristically Celtic, not Anglo-Saxon-Norman. And, in any event, we can (and shall) demonstrate that only a small minority of the “Celtic” population of the antebellum South was of Lowland origin.

Two: Rather more, but (as Ian C. C. Graham has shown) still a small, portion of the Scots in America, came from the Highlands. Professor Berthoff is dreadfully confused on the subject of the Highlanders, especially in regard to their supposed antipathy to pork. It is true that some Highlanders were averse to eating the flesh of pigs, but several points need be made: (1) It is from the accounts of outsiders, not from Highlanders themselves, that we are told of the prejudice. It was easy for

travelers to be misled in this regard, for a peculiar linguistic reason. The Gaelic word for hog is *obhaisg*, and that for a yearling ewe is *othaisg*; in Scottish English “hog,” to which Pennant and many other travelers report the Highlanders’ aversion, meant sheep under the age of one year, or lamb. Thus in either language, travelers being told of Highlanders’ prejudice against lamb could mistakenly have heard and reported that the Scots were prejudiced against pork. Southerners, by and large, refuse to eat lamb to this day. (2) Captain Edward Burt in his *Letters from a Gentleman in the North of Scotland* (1754), 123–24, did refer specifically to a supposed prejudice against pork, deriving from following the examples of particular clan chieftains; but he says that even members of those clans would eat pork when it was available, if the chief was not present. The real reason for the relative absence of the meat, Burt pointed out, was that it was too expensive to feed pigs. That is quite a different thing from a cultural aversion to eating pork. Incidentally, Martin Martin said that the islanders were very fond of whale meat, and called it “sea-pork” (Martin, *Description of the Western Islands* [1716], 5–6); (Muc = sow or pig, and muc-mhara = whale). (3) Anyone having even a passing acquaintance with the economic and social history of the Highlands in the eighteenth century knows that such prejudice against swine as the Highlanders did have was overcome as soon as it became practicable—once again—to feed the beast. In earlier times, when the area was forested with hardwoods that furnished mast, the Scots had eaten pork with all the enthusiasm of their Irish ancestors. Then the forests were stripped, in most places, and there the pigs virtually disappeared. But, upon the introduction of the potato into Scotland during the last half of the eighteenth century, there was suddenly a cheap way to keep pigs, and the Scots—given a way of raising pigs—quickly regained their traditional relish for the animal’s flesh. (4) Pennant and various other travelers recorded the raising of large numbers of swine by the open-range in some parts of the Highlands. (At the Atlanta Southern meeting, Mr. Berthoff insisted that Caithness, one county regarding which Pennant made such an observation, was not a part of the Highlands. During the summer of 1981, we journeyed extensively through Caithness, and if that is not Highland country we cannot believe either our eyes or anyone we talked with in the region. By the way, open-range livestock-raising still exists there today, as it does elsewhere in Scotland, Ireland, Cornwall, and North Wales. By the way, also, we have spent a good deal of time in the West of England, as well as in Virginia, and we still trust our ears.)

Three: Most of the Southerners of “Celtic” extraction were Scotch-Irish. It is conventional to believe—and, for a long time, we mistakenly believed—that the Ulster Scots who emigrated to America during the eighteenth century were descendants of Lowland settlers upon the plantations established by James VI/I. The fact is that most Ulster immigrants to America in the eighteenth century, as Dickson has shown, moved from the counties of Antrim and Down—where there had been no Stuart plantations. Those two counties had long formed a part of a culture based in the Hebrides and the north of Ireland. In Antrim and Down, Scottish land titles had been confirmed (by John I) at least as early as the first quarter of the thirteenth century; and until the late seventeenth century, the inhabitants were regarded by the English as the “most Celtic” of peoples in the

British Isles. These were the people who settled colonial America south and west of Philadelphia: these were our lazy, or leisure-oriented, antebellum Southerners.

A few odds and ends. We do not think we have violated the spirit of the sentence we have quoted from Professor J. G. A. Pocock; we refer Professor Berthoff to page 6 of that article and suggest that he reread pages 7 and 10, for in all of those places Pocock used the term "Celtic" precisely as we have used it. A. L. Rowse, while being vehement in his statements that "to be Cornish is a very different thing from being English," easily and frequently referred to his Celtishness and the Celtic heritage of Cornwall (see, for example, *A Cornish Childhood*, 8, 10, 61, 68, 91, 93, 107). Berthoff might also consult the works of the brilliant sociologist Michael Hechter, who employed the term Celtic and demonstrated its behavioral validity even unto this day. We further refer Mr. Berthoff to the *Oxford English Dictionary*, *Webster's Third Unabridged Dictionary*, and the *Encyclopaedia Britannica*, all of which use the term "Celtic" to encompass the peoples we are describing. Perhaps the editors of the above have already received angry letters from Mr. Berthoff.

We could go on and on. Indeed, you may count on it that we shall do just that. We have been accused of many things, but no one has accused us of lacking confidence in our interpretation. Our attitude, we think, has some foundation: what we have going for us is history and common sense.

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DESPITE THE COMMENTARIES by William H. Sewell, Jr., and Sylvia L. Thrupp on Alette O. Hill and Boyd H. Hill, Jr.'s "Marc Bloch and Comparative History," I think that some further comment is necessary from the point of view of the history of linguistics and of anthropology.

The Hills began their discussion of the comparative method in linguistics with Antoine Meillet's *La Méthode comparative en linguistique historique* (Paris, 1925), because in their article Bloch is said to have drawn his ideas from it. From there on, they relied almost exclusively upon French writers, especially Meillet and Ferdinand de Saussure, in their analysis of the history of linguistics. They ignored the whole earlier history of the adulation of, revulsion from, and modified use of one or more forms of the comparative method in linguistics and anthropology. In linguistics, the debate largely originated with German writers, such as F. Max Müller, August Schleicher, Johannes Schmidt, Berthold Delbrück, and Franz Boas. But these men's analyses, evaluations, and criticisms of comparative methods, which had profoundly altered attitudes by Meillet's time, were not mentioned. The Hills'

citation of only one general history of linguistics (p. 833, n. 23), Holger Pedersen's rather narrow and old-fashioned *The Discovery of Language*, written basically in 1924 and translated for publication in 1931, revealed no great familiarity with the abundant recent literature on the history of linguistics. (See Hans Arens, *Sprachwissenschaft* [2d edn., Freiburg, 1969], and Thomas A. Sebeok, ed., *Current Trends in Linguistics*, 13 [The Hague, 1975].)

The Hills subscribed to Meillet's subdivision of comparative method into two types: one that produces universal (or typological) laws, and the other that reconstructs historical data and is based on the assumption that the compared items have a common historical or prehistorical origin. But at least three comparative methods were noted by writers in the 1860s: (1) a universal one that describes general similarities or differences based on comparison of any items, (2) a historical one (also called the comparative-genetic method and associated chiefly with the new nineteenth-century discipline of comparative Indo-European linguistics) that compares only items believed to be historically and "genetically" related and yields reconstructions of a historical or hypothetically historical kind, and (3) an evolutionary comparative method that links by analogy general similarities or differences in a typological (see method 1) or a "historical" (or historicist) evolutionary sequence (see method 2), despite the lack of direct historical evidence for such a sequence. The third, evolutionary method was in use considerably before the nineteenth century, but it was brought to the fore by Auguste Comte and later sociologists, such as Herbert Spencer, as well as by evolutionary anthropologists, such as L. H. Morgan, J. Lubbock, J. F. McLennan, and E. B. Tylor.

This evolutionary or developmental use of comparative method, although it was one of the most important and was the one that most directly led to the late nineteenth-century reaction against comparative method by Henry Maine, Boas, and others, is not explicitly mentioned in the Hills' article. Yet one of its practitioners—J. G. Frazer—is criticized in Bloch's discussion of comparative methodology, which the Hills quoted. In my recent book, *Culture in Comparative and Evolutionary Perspective: E. B. Tylor and the Making of Primitive Culture* (West Berlin, 1980), I have analyzed these three versions of the comparative method, their alternating use, and their confusion and mixing by some nineteenth-century linguists and anthropologists (much as by Bloch later); also included are discussions of contemporary and modern criticisms of these methods and extensive bibliographical footnotes on the theme of comparative method.

An analysis of Bloch's and others' use of comparative method requires more refinement than the Hills' article gives to it. One issue is the number of comparative methods that Bloch recognized. Rulon Wells has recently postulated at least four comparative methods (Wells, "Linguistics as a Science: The Case of the Comparative Method," in Henry M. Hoenigswald, ed., *The European Background of American Linguistics* [Dordrecht, 1979], 29–31). The Hills indicated that Bloch basically assumed that there were two comparative methods, but that he may have meant more than two when he wrote of "la plus saine méthode comparative." This phrase may not, however, be a superlative comparing at least three methods, as the Hills have interpreted it to be, but an absolute superlative. In any case, the issues

involved in Bloch's supposed definition of two comparative processes or methods (p. 830) concerned not so much these different types of comparative method as they concerned the related, but more general and longstanding, anthropological debate about the relative importance of two (as Bloch divided them) explanations of cultural similarities: (1) parallel, independent invention (Type I), and (2) common origin and diffusion (Type II). Bloch's choice of an example of Type I from Frazer and his adjoining criticism of Frazer's attempt to find a common psychological basis for similar religious rites among diverse peoples indicate that Bloch was addressing a general anthropological (and thereby historical) problem of methodology rather than a narrowly linguistic one. As a historian, Bloch would, like most historical linguists, by professional definition have opted for historical comparisons (method 2); he did nominally opt for them, although he did not necessarily make modern European comparisons on a strictly "genetic" Indo-European basis.

The reaction against the comparative method was, in some cases, a reaction against the genetic comparative method, which was helping establish unilinear lines of development, single centers of early high civilization, and "racial" classifications and hierarchies based upon language, such as the Aryan theory of race. Unfortunately, the confusion of language, nation, and race in such theories was relegated to a footnote in the Hills' treatment, and the reaction, including Bloch's, to these by-products of the comparative method was largely ignored. Part of this reaction manifested itself in the increasing importance of the study of linguistic geography—especially outside of Germany—in France, Switzerland, Denmark, and Great Britain. In this field, linguistic, ethnic, and social diversity as well as the influence of diffusion could be stressed in a positive fashion. The Hills, however, considered dialect geography to be just another application of linguistics' comparative method; they said that the techniques of dialect geography could equally well be applied to the whole Indo-European language "family" by Meillet as to a Swiss canton by Jules Gilliéron (p. 832). In fact, most dialect research did concentrate upon smaller linguistic groups than the Indo-European "family" of languages and was committed to local studies, which emphasize diffusion, convergence, and other historical changes; these studies confuse the search for common "origins" and for the continuity of prehistoric characteristics, the goal of much traditional historical linguistics and anthropology. Bloch himself also stressed the problem of "mixing," a problem that, according to Meillet and many others, destroys the efficacy of the historical comparative method. Therefore, one might expect that Bloch's use of the historical comparative method would have been tempered by the diffusionism and methodological implications of the geographically oriented studies in linguistics and history that were of importance to him and his era.

Many examples can be found of the Hills' myopia, their too exclusive reliance upon (often French) post-Saussurean sources and thus upon hindsight. One such case is their dismissal of Max Müller, the first professor of comparative philology in England (1868), as one who "was considered a popularizer at best," whose work was "erroneous" and "dubious" (p. 838, n. 37). Müller's editions of Sanskrit texts are certainly not of this character, although his more popular lectures on the "science of language" and his search for prehistoric "Aryan" words and characteristics in

modern languages and peoples did lead him and many others to imaginative excesses of linguistic and “racial” interpretation. But it was his imaginative excess and felicitous style that led his contemporaries in other fields—for example, Maine and E. A. Freeman—to adopt what he presented as linguistics’ comparative method and Aryan theory in their comparison of societies. That Maine did not wait for the more cautious and rigorous Neogrammarian period to adopt the comparative method, as the Hills lamented, is self-explanatory. The caution and narrower professionalism of the Neogrammarians as well as the publication of their works almost exclusively in German meant that, unlike Müller, they did not attract a popular audience. Their works would have been unintelligible to outsiders, who, if interested, had probably already been revved up by a writer like Müller. And by the Neogrammarian period, the novelty and reliability of several versions of the comparative method were being challenged. After Maine, the next figure whom the Hills mentioned as taking up the banner of linguistics in another discipline was Claude Lévi-Strauss (pp. 838–39). Certainly, this was quite a gap and should not be skimmed over. Although Lévi-Strauss’s praise of the scientific quality of linguistics does seem reminiscent of that of Müller and similar writers, one should not place Bloch on a continuum between the two. The prestige of linguistics has not been equally high or its influence equally pervasive in all of the intervening periods.

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JOAN LEOPOLD’S LETTER TO THE EDITOR about our article on Marc Bloch in no way disturbs our thesis that Bloch said he intended to follow the model of the linguists like Meillet (Type II) rather than the universal approach as exemplified by Sir James Frazer (Type I) and that he failed to carry out this venture in any systematic way. Moreover, Professor Leopold introduces a number of errors and irrelevancies into her discussion. For example, she writes, “The Hills began their discussion of the comparative method in linguistics with Antoine Meillet’s *La Méthode comparative en linguistique historique* (Paris, 1925), because Bloch is said to have drawn his ideas from it.” Bloch is *said* to have drawn upon this book? He cited it in the notes to his article “Pour une histoire comparée des sociétés européennes”: “Voir surtout A. Meillet, *La méthode comparative en linguistique historique*, 1925, auquel j’emprunte l’idée générale du développement sur les deux formes de la méthode” (*Revue de Synthèse Historique*, 46 [1928]: 16 n. 1). Perhaps Leopold’s use of the passive voice in this sentence derives from her failure to check the source.

In another passage Dr. Leopold misconstrues our analysis of Bloch and substitutes her own: “Bloch’s choice of an example of Type I from Frazer and his adjoining criticism of Frazer’s attempt to find a common psychological basis for similar religious rites among diverse peoples indicates that Bloch was addressing a general anthropological (and thereby historical) problem of methodology rather

than a narrowly linguistic one.” (1) Bloch did not criticize Frazer; he praised him—a fact that can be verified by consulting the source; (2) a “general anthropological problem” is not *ipso facto* a historical one; and (3) we did not infer and then imply that Bloch was “concerned with a narrowly linguistic [methodology].” We wrote that Bloch stated that he *preferred* it to the methodology of Frazer (p. 831).

Leopold also misreads and misinterprets our discussion of “mixtures” in linguistics (p. 833). She states that according to Meillet “mixing [*sic*] . . . destroys the efficacy of the historical comparative method.” In reality Meillet said that, when morphological mixtures are found (none had been), “linguistics will have to devise new methods.” By substituting the word “mixing” for “mixtures” she makes it unclear whether she means a conflation of anthropological methods or a hypothetical morphological entity. On the basis of her erroneous premise (that mixtures had been found), Leopold jumps to the conclusion that “Bloch’s use of the historical comparative method would have been tempered by the diffusionism and methodological implications of the geographically oriented studies in linguistics and history that were of importance to him and his era.” This extrapolation implies that “geographically oriented studies in linguistics” dispensed with the comparative historical method because its efficacy had been destroyed by “mixtures,” even though no such event is attested. Thus, she has replaced history by fantasy.

One of her most revealing accusations is that we ignored those linguists who antedate the Neogrammarians. Quite true; but, since we were writing about Bloch, and since Bloch relied upon Meillet, there was no purpose in mentioning August Schleicher and his pupil Johannes Schmidt. We did not intend to present a history of the comparative method. Otherwise, we would have begun with Sir William Jones, Rasmus Kristian Rask, and Baron Cuvier. Where she alleges that the work of Schleicher (1821–68) and others had profoundly changed attitudes toward the comparative method by Meillet’s time, she appears to have turned chronology on its head, as if the early comparative linguists had swept away the work of the Neogrammarians who followed them in the last quarter of the nineteenth century. Moreover, Leopold puts Berthold Delbrück (1842–1922) in the same group with Schleicher. Yet Delbrück wrote the volume on syntax for Karl Brugmann’s *Grundriss der Vergleichende Grammatik der indogermanischen Sprachen*, and Brugmann (1849–1919) was the most prominent of the Neogrammarians and wrought the fundamental changes in Schleicher’s treatment of Indo-European *Ablaut*. By lumping together Müller, Schleicher, Schmidt, and Delbrück, Leopold grossly distorts the historiography of linguistics, and by adding Franz Boas (1858–1942) to this eclectic group she boggles the reader’s mind. It is also noteworthy that Franz Boas and Max Müller are classified as “German writers” here, yet later on Müller’s popularity is ascribed to his ability to “rev up” a broad English-speaking following because he did *not* write or lecture in German.

Leopold condemns Holger Pedersen’s *Discovery of Language* (1924) as “rather narrow and old-fashioned,” although this book reflects the views of Indo-European linguists at the precise time that Bloch came under their spell. Pedersen’s fact-filled compendium can scarcely be described as “narrow” unless narrowness be construed as the omission of any reference to Max Müller, Leopold’s favorite philologist.

Pedersen did include Henry Sweet (Shaw's model for Professor Higgins in *Pygmalion*), proving that he did not eliminate scholars simply because they produced popular books.

Dr. Leopold misreads our essay once again in the following: "The Hills subscribed to Meillet's subdivision of comparative method into two types . . . but . . . at least three comparative methods were noted by writers of the 1860s." It was *Bloch*, not the Hills, who subscribed to Meillet's dichotomy; if he had appropriated the three methods popular in the 1860s, we would have focused our attention on them. She further points out that at least four comparative methods have recently been postulated by Rulon Wells. By our failure to cite this work she concludes that our analysis of Bloch "requires more refinement than the Hills' article gives to it." Since Bloch mentioned only two methods when he wrote on this subject in 1928, it is hardly pertinent that four have now been delineated. In fact, Wells has not "postulated at least four comparative methods." Rather, he listed four "senses" in which the word "comparative" is used: "(1) the literal, etymological sense, (2) the sense current in biology, (3) the sense current in anthropology, and (4) the sense current in linguistics. (Logically the senses are not coordinate; the last three all fall under the first, as species, or subspecies, in a 'tree of Porphyry')." (See "Linguistics as a Science: The Case of the Comparative Method," 29.) Throughout his article Wells referred to one and only one comparative method, which leads us to suggest that Professor Leopold could do with a bit of "refinement" herself.

In her treatment of the "racial" overtones of comparative methodology, Leopold chides us for relegating "the confusion of language, nation, and race" to a footnote. On the contrary, we said in the body of the article, "There is, however, no *necessary* connection between language and ethnic origin," to which we appended a footnote referring the reader to "a forceful corrective to the older historiography, which tended to confuse language, race, and nation" (p. 831, n. 16).

We are charged with considering dialect geography "just another application of linguistics' comparative method," as if we did not accord this important field its due. We wonder if Professor Leopold really understood the pages on which we underscored its significance (pp. 832–33) and the footnote that refers to the work of Pierre Goubert, who showed that certain long-held assumptions were being challenged by local historians (p. 832 n. 21). Our purpose in stressing the parallelism between dialect geography and local history was that both were relevant to our discussion of Bloch.

We are diagnosed as being afflicted with "myopia" in our "too exclusive reliance upon (often French) post-Saussurean sources." It is true that Saussure wrote in French and so did his pupils, but we had no hand in this conspiracy. Saussure laid the structural foundations of modern linguistics, including the diachronic branch with which we were most concerned (p. 840, n. 46). Saussure and his followers are therefore crucial to an understanding of developments in twentieth-century linguistics, and we did not feel it necessary to apologize for the fact that they wrote in French. But it is not only the French scholars who are suspect: Professor Leopold turns her guns on the Neogrammarians, who, unlike the Anglophile Müller, wrote in German!

We do not retract our characterization of Max Müller as “a popularizer at best,” Müller being the man whose reputation Professor Leopold seems most anxious to salvage. Even his obituary in *Nation* did not gloss over his plagiarism and egomania, though granting him high praise for enthusiastic lectures to the general public, which sparked interest in Sanskrit literature in translation and “Aryan” mythology. (See E. W. Hopkins, “Max Müller,” in Thomas A. Sebeok, ed., *Portraits of Linguists*, 1 [1966]: 395–99.) Müller appears to figure in Leopold’s new book on E. B. Tylor, which is prominently described in her letter. What she has to say about our article on Bloch pales in significance beside her own achievement. We can hardly wait to read her tome, since by the author’s own admission it “analyzes . . . three versions of the comparative method, their alternating use, and their confusion and mixing by some nineteenth-century linguists and anthropologists (much as by Bloch later).” Her book may indeed analyze three types of comparison, but it will not change the fact that Bloch mentioned only two, chose one (Type II), and failed to pin down his use of it in any precise manner.

In short, Professor Leopold seems to have taken our article as an opportunity to advertise her own work rather than to contribute anything new to Bloch scholarship or comparative linguistics. This practice is now so common that it has become a recognizable genre. For a splendid example, see Professor Jonathan M. Wiener’s witty rejoinder to the critics of his *Forum* article, “Class Structure and Economic Development in the American South, 1865–1955” (*AHR*, 84 [1979]: 970–1006). On page 1003 Wiener nicely demolished Robert Higgs’s argument by drawing attention to the practice of “self-citation.”

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Reviews of Books

GENERAL

DAVID RUBINSTEIN. *Marx and Wittgenstein: Social Praxis and Social Explanation*. Boston: Routledge and Kegan Paul. 1981. Pp. 231. \$25.00.

The present volume is an attempt to contribute to the methodological reflections of contemporary social science by identifying some alleged parallels between the thought of Marx and that of Wittgenstein, which (according to the author) help to resolve some characteristic impasses in social methodology.

David Rubinstein's book begins with a dichotomy between "objectivism" and "subjectivism" in social science. The former, derived from the positivist tradition, attempts to explain social behavior in terms of "objective" factors independent of human consciousness. The latter, by contrast, tries to understand society in terms of the meanings with which the human mind invests it. The book's central contention is that neither extreme is satisfactory and that both fail because they presuppose a common doctrine: a "dualist" theory of mind. (In this book, "dualism" apparently refers not merely to the doctrine that minds and bodies are distinct substances but to any theory of mind that involves the idea that mental phenomena are in any sense "private," "inner," or open to "privileged access.") The author contends that Marx and Wittgenstein are at one in rejecting "dualism" in this sense, and this is the basis of their contribution to the resolution of disputes between "objectivism" and "subjectivism."

Part 1 of the book is a critical discussion of "objectivism" and "subjectivism." Part 2 is a comparative exposition of the views of Marx and Wittgenstein on a number of topics relevant to these approaches and the common errors they presuppose. According to the author, Marx and Wittgenstein hold a theory of mind according to which "mind is not a feature of consciousness but of action" and "understanding human actions requires an analysis

of their social context" (p. 2). They agree that all mental phenomena are "internally related" (or "conceptually connected") to a "context of social action" that gives them their meaning. Because Marx and Wittgenstein are right, the task of social science should be to understand social behavior in terms of its "meaning" (as "subjectivists" contend), but this understanding is not to be conceived as an "empathy" with the inner states of others, nor is meaning something "inner" which receives an outward expression in actions or objects (as "subjectivists" mistakenly suppose). Instead, understanding involves grasping the "system of social praxis" (p. 204) in which an action or mental phenomenon must be involved. Objectivists have gone wrong in conceiving such a system as independent of the mental, whereas in truth (as Marx and Wittgenstein allegedly teach us) this system as an object is "mediated by the properties of the subject" (p. 166).

In the judgment of this reviewer, the present volume does not make much contribution either to the exegesis of Marx and Wittgenstein or to the methodology of social science. The interpretations of Marx and Wittgenstein are largely derivative and not especially lucid. The interpretation of Marx strikes me as especially open to question. Marx certainly had interesting views about the methodology of social science, but he probably never asked himself most of the questions about the philosophy of mind with which this book is concerned. The author indulges in the all-too-common habit of wringing and twisting the familiar juicy quotations from the Marxian corpus in order to extract the "right" answers to whatever questions the author finds pertinent. For example, the author realizes that Marx often seems to endorse an "objectivist" program in social science, insofar as he proposes to explain "consciousness" in terms of "social being" rather than the reverse. The author rebuts this interpretation of Marx not by citing any texts to the contrary, but by contending that on an "objectivist"

reading Marx's theoretical program would be defective. Many writers have used similar arguments to show that Marx's program is in fact defective. The author says nothing, however, to exclude their inferences to this conclusion.

The author's philosophical bias against "dualism" (the general idea that some mental phenomena are "inner" and that consciousness has "privileged access" to them) is not defended with any originality or philosophical sophistication. The book's critique of "dualism" may seem right-minded to ardent devotees of the later Wittgenstein, but I doubt that it will appear so to anyone else (and even such devotees will not find anything new or especially insightful in it). The alternative view of mind is not worked out with much clarity or sophistication either. The author rather oddly repudiates any form of "behaviorism," which, he contends, is an "objectivist" position. (This claim is made out successfully using Skinner's rather idiosyncratic form of behaviorism, but the author does nothing to show that it is true of other forms, or indeed to rebut the suggestion that his own view might be described as "behaviorist" in some plausible sense). The author is aware that his own methodological proposals are not new, and not unique to Marx or Wittgenstein. He correctly cites Dilthey and Mead, among others, as exemplifying the sort of method he favors. But he takes both these theorists to task as "subjectivist" for viewing understanding as "empathy" (Dilthey) and "reverting" to the idea that "action is an expression of an inner state" (Mead). He does not, however, show how these philosophical sins do any harm to Dilthey or Mead's social method. In fact, I am inclined to think that both thinkers could be rescued from "subjectivism" altogether if we allow them the same principles of exegetical charity employed by the author in rescuing Marx from "objectivism."

The strong point of this book is the extensive knowledge of a wide variety of literature (in sociology, philosophy, and scholarship on Marx and Wittgenstein) which it exhibits. In all candor, however, I cannot see very much new light shed either on Marx, or on Wittgenstein, or on social method by all this erudition.

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BRIAN EASLEA. *Witch Hunting, Magic, and the New Philosophy: An Introduction to Debates of the Scientific Revolution, 1450–1750*. (Studies in Philosophy, number 14.) Sussex: Harvester Press or Humanities Press, Atlantic Highlands, N.J. 1980. Pp. xii, 283. Cloth \$42.50, paper \$16.50.

The chapter titles of this book promise an investigation of the mutual effects of witchcraft, cosmology,

matter-theory, the idea of gravitation, and "the appropriation of nature" in the early modern period, but in fact the great bulk of the volume offers nothing so original, filled as it is with potted summaries of well-known problems and persons. Fewer than twenty of Brian Easlea's five-hundred-odd notes make direct reference to sources not in English, and primary texts are usually cited second-hand. Even the English secondary sources on which the book almost entirely depends are poorly digested; as an instance, compare Easlea's remarks on Reginald Scot, which ignore the crucial problem of Old Testament exegesis, with Sidney Anglo's excellent article in *The Damned Art*. This is surely not the "critical account" advertised in Easlea's introduction, nor can it be recommended as an "introductory text."

Easlea has taken on much more than he understands, especially in the difficult area of magic. At one point he makes the idea of occult qualities "anti-Aristotelian," a blunder that may perhaps be explained by the book's failure to deal with the Peripatetic doctrine of substantial forms, which long formed the philosophical basis for belief in occult qualities. Natural magic turns out to be an anti-Christian force in Easlea's scheme of things, although Marsilio Ficino—priest as well as magus—would have reasonably objected to the charge. Throughout, Easlea uses terms like "Aristotelian," "Thomist," "Hermetic," and "Neoplatonic" with great abandon and little precision. The constant and inexpert shuffling of these and other categories leads to some difficulty in keeping the good guys apart from the bad and calls up formulations like the following: "Misogynist though he was, Paracelsus was a man of the people" (p. 102).

Misogyny as a dominant element in the scientific revolution is one of Easlea's principal themes. Once Descartes had deprived matter of its "interesting properties," he argues, matter became passive enough to serve as a conceptual target of opportunity for "mechanical philosophers" anxious to display intellectual virility. The Cartesian account of collision thus becomes sexually meaningful. Lest I be accused of exaggerating the moral ferocity of Easlea's message, let me quote him at greater length: "The new philosophy has restored autonomy to man in his confrontation with nature and man will triumph. . . . Whether he will triumph as a mechanic over an engine, a man-midwife over a woman in labour, a seducer over a mistress, or as a rapist over his female victim, he will triumph" (pp. 216–17). I find this to be a thoroughly misbegotten picture of a revolution in human thought that began with Leonardo da Vinci and ended with Isaac Newton.

BRIAN P. COPENHAVER
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LEONARD W. LEVY. *Treason against God: A History of the Offense of Blasphemy*. New York: Schocken. 1981. Pp. xviii, 414. \$24.95.

Leonard W. Levy's major contributions prior to this book have been in the area of the American Constitution. Perhaps the present effort is an extension of an interest in the origins and present state of First Amendment problems. The subject of blasphemy and its suppression is really not so far-fetched for one committed to intellectual and religious liberty. As Levy affirms, "in our time, as in the past, this is not a subject of merely academic interest." Indeed, "the use of the criminal law to assuage affronted religious feelings imperils liberty—not greatly, to be sure, because blasphemy prosecutions have become legal relics in the Anglo-American world. But they are reminders that the feculent odor of persecution for the cause of conscience, which is the basic principle upon which they rest, has not yet evaporated."

Levy traces the origins of the offense of blasphemy to Mosaic tradition as especially located in Leviticus 24:16—"He who blasphemes the name of the Lord shall be put to death." Rarely put into effect during ancient Jewish history, after the trial of Jesus and the later successes of Christianity the concept was greatly expanded and elaborated. With this expansion Levy is more concerned.

The impression and unevenness of definition are correctly noted. Throughout the history of blasphemy and its suppression an interchangeable word has been "heresy." And it is obvious that "heresy" covers everything from politics to the denial of the doctrine of the Trinity. During the ancient period as well, prior to Christianity's legalization (and secularization), heresy to one was orthodoxy to another. Definition of heresy and blasphemy was nearly impossible at this time; it was always contextual and political. Once there was *the* universal church (after the fourth century, essentially), definition and imposition were possibilities. Levy really never disentangles blasphemy and heresy—perhaps this is an impossibility. He does decide to spend little time with the medieval period because the crime then was "heresy." He probably quibbles at this point. Although the word "blasphemy" was revived in the Reformation and extended during the modern period, heresy and blasphemy continued to mean about the same thing in the long run. Levy's disclaimer that "this is not a book about heresy" is hardly true. It is a book about heresy and blasphemy but with little space given to the Middle Ages.

The strongest sections of the book are found in those chapters that settle in with post-Reformation England, where in 1676 blasphemy was judged officially to be a crime against the state. Heresy,

blasphemy, nonconformity—all are nearly synonyms. Socinians, Antinomians, Quakers, and numerous other religious minority groups were persecuted as enemies of the state. Levy deftly traces the story in English history of this crime against the state.

Levy's book is "must" reading in our times of the revival of fundamentalism and authoritarianism in religious circles. Moral Majority *et al.* would revel in more "fires of Smithfield." This book is at least a forewarning of such dangerous groups in our society and the need for their exposure.

GEORGE H. SHRIVER
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ROBERT G. FRANK, JR. *Harvey and the Oxford Physiologists: Scientific Ideas and Social Interaction*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xviii, 368. \$27.50.

This book evolved from Robert G. Frank, Jr.'s thesis and is undoubtedly one of the most readable and well-organized books of this genre in the history of science. Relying mostly on original sources, many of them written or published in Latin, the author has interpreted many physiological tracts virtually unavailable to the modern reader. He has also placed them in context, so that anyone wishing to understand one of the major scientific discoveries of the period may now do so with the least amount of effort.

The less-than-four-page preface should be read by historians of every persuasion for the clarity with which it explains one approach of the historian of science to seventeenth-century events. Combining fluency in technical details with awareness of interpersonal relationships and individual and institutional behaviors and idiosyncracies, Frank has skillfully constructed a history of the adoption of the idea that the blood circulates throughout the body, with all the physiological consequences stemming from this fact. There is no finer source that explains to the reader the ideas and functions of the group of experimentalists and physicians in the Oxford-London region who taught one another about the complexity of the circulation of the blood between 1640 and 1675.

Special features of the book include a table of Oxford scientists and virtuosi (1640–75) on pages 63–89 and a plethora of details about the landmarks of Oxford, the conventions of book production and sales, and the specific avenues of exchange between the Oxford experimentalists that shed light on their interaction for scientific purposes.

Frank shows how, for instance, in the case of Nathaniel Highmore, physiological explanations changed from their original form. In systematizing

Harvey's deductions, Highmore introduced a particular interpretation of the blood, although Harvey emphasized the unitary and vital nature of the blood. Another shift Highmore made was to use different terms for the major artery and vein connected to the heart. Instead of *vena arteriosa* (artery-like vein) and *arteria venosa* (veinlike artery) he used the terms *arteria pulmonum* and *vena pulmonum*, in keeping with the concept of the circulation. It was with shifts of this type that the circulation doctrine gave rise to revisions in anatomy, physiology, and (as witnessed by William Petty's lectures of 1650) pathology.

Frank places into historical context the scientific perambulations of the seventeenth century and relates them to later events when appropriate; throughout he shows awareness of the limitations of this practice, keeping in mind some of the main elements of confusion and misinterpretation. One example of a brilliant discussion of this type occurs on pages 241–44, where Frank sums up the concept and discovery of aerial nitre, which has been considered (incorrectly) the prototypical form of oxygen by some historians of science who credit John Mayow as the discoverer of oxygen one century prior to Antoine Lavoisier, the legitimate claimant to this honor.

One imagines the applicability of Frank's multifaceted approach to the people, experiments, ideas, and institutions of seventeenth-century Oxford to other scientific groups, periods, and problems. The historical method evolved here will undoubtedly be tried and proven illuminating, and at the same time it should lead to other conclusions about the nature of the scientific enterprise. The major concepts of Oxford scientist-physicians are well represented here: including other contemporary institutional gatherings and exchanges, however, seems to offer scope for an even broader historical assessment of science in this period. The Continental groups working on problems growing out of the circulation doctrine now deserve study, not only to link them up with the Oxford group, but also to show the universal nature of the scientific enterprise even in its early stages. The details gathered and arranged with such meticulous care in Frank's book lead me to speculate on the possibility of broadening the scope of this text to include the work of all or many more individuals of the British scientific community of the period and to use the existing secondary literature, not just as references in footnotes, but to harvest their riches at appropriate junctures as Frank has laid up his own stores from the fields of original sources he has tilled, planted, and fertilized. Frank has added another lens through which our views of the seventeenth-century revolution may be clarified, including the insight that when science

ceases to grow it is not true science. Therefore the historian must be attuned to the signs of decay as well as novelty and advancement in scientific discussion and discovery, and attuned also to the vagaries of human energy and interaction in cataloguing all of it.

AUDREY B. DAVIS
Smithsonian Institution

KENNETH H. F. DYSON. *The State Tradition in Western Europe: A Study of an Idea and Institution*. New York: Oxford University Press. 1980. Pp. viii, 310. \$19.95.

The first sentence of this book announces that it was not easy to write. Nor is it easy to evaluate. Kenneth H. F. Dyson displays a fine sense of the difficulty of his subject when he emphasizes early on that the Anglo-American academic world is not familiar or comfortable with the concept of "state" but that it is nonetheless important and not only in the Continental tradition. "State" is partly a mental construct of broad influence and partly a technical term covering both the complex of authoritative institutions (legislative, judicial, and executive) and the array of sentiments expressed by officials and the public when they refer implicitly or explicitly to the "dignity" of those authoritative institutions. The author deals with this array of topics under three headings: "The Historical Tradition of the State," "The Intellectual Tradition of the State," "State as an Ideal Type and as a 'Problem-Defining' Concept."

Under these headings he covers a large range of topics. For example, chapter 2 deals successively with the concepts of state and society, the crisis-of-modernity polity, the transition from feudal to clientelist politics, balanced parliamentary and dual politics, and finally adversary, authoritarian corporatist, and accommodative politics. He classifies these topics in a sixfold table, dividing types of state along the axes of *Gemeinschaft* and *Gesellschaft* and of "stateless" and state politics. (Incidentally, it is unclear why "stateless" deserves quotation marks of mental reservation, while state does not, when the whole book deals with the manifold meanings of the latter term.) From this chapter of thirty pages the reader will gather that a systematization of historical types is attempted, not a discussion of institutional history. Indeed, the author's intention is clearly to stick with the "idea of the state" as his central topic, so that a more appropriate title of the book might be *The Intellectual Tradition of the State in Western Europe*. Yet Dyson does not write intellectual history either, or only intermittently. The chapters of part 2, extending over 120 pages and clearly the central part of the book, deal successively with "The State and the Intellectuals," "Three Conceptions of the State: Might, Law, and Legitimacy," "The Historical

Development of Theories of the State," "A Comparison of Theories of the State in France and Germany," and "The Word 'State' in the British Intellectual Tradition." This suggests a sociological, conceptual, and comparative framework. Since part 3 also deals mostly with conceptual questions, one must conclude that the subtitle of the book is misleading. Dyson has written the study of an idea only, not of an idea and institution.

What then is the quality of this analysis of the "idea of the State"? To put the positive side first and give it due emphasis, the author *has* covered the literature in comprehensive fashion. I have been unable to discover any important writer on the political theory of the state who has been omitted, although the emphasis is on writers since the eighteenth century. In this respect it may be most appropriate to regard the work as an expanded handbook in which the interested reader, by consulting the table of contents and the index, can easily locate the pertinent authors and discussions of every aspect of the literature. From this it is clear that the material is organized in thematic fashion so that the same authors are referred to repeatedly. Dyson's analysis comes alive most clearly in chapters 6 and 7 when he compares the French, German, and British approaches to the state. But on the whole he achieves his impressive comprehensiveness at the price of not having enough space for a more detailed consideration of any one author, some of whom are clearly more important than others.

I am sure it was difficult to write this book, but I confess it was also difficult to read it. The author's style is not at fault, but perhaps his decisions concerning the organization of his material are. The interested reader will find the book most useful as a reference work with regard to specific topics concerning the idea of the state. The author provides a good introduction to the many facets of this literature and hence a useful guide to further reading.

REINHARD BENDIX
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JAROSLAW PELENSKI, editor. *The American and European Revolutions, 1776-1848: Sociopolitical and Ideological Aspects*. Iowa City: University of Iowa Press. 1980. Pp. xviii, 412. \$17.50.

With events in Poland dominating today's news, the appearance of this book could not have been more timely. It is the published version of the second conference of Polish and American historians that took place at the University of Iowa during our bicentennial year. The general theme of the conference dealt with the American and European revolutions, but the title does not do full justice to the

diversity of topics covered. There are sixteen separate articles here as well as an extensive introduction. These range from ethnicity and religion and the question of women during the American Revolution to the partitions of Poland, and from the French Revolution to Ukrainian peasant rebellions. Prominently featured is the rather checkered career of Tadeusz Kosciuszko. Nevertheless, because of the general lack of knowledge in the West of Poland's history, the main achievement of this volume is its presentation of various aspects of the Polish heritage to an English-speaking audience. Moreover, the book does provide us with an opportunity to examine the quality of Polish historical scholarship.

R. R. Palmer once praised Polish historiography as being "the best that has been done in any of the countries where a Marxist frame of history is officially prescribed." Although this reviewer is not in a position to rank levels of scholarly endeavor produced in all communist societies, there can be no doubt that some of the Polish contributions to this collection are first-rate, whatever the criteria one uses. I would single out for special praise Andrzej Walicki of the Polish Academy of Sciences, whose article on revolution in Polish thought can be favorably compared with anything done in the West on Russian populism, including the work of Franco Venturi. Of particular interest in Walicki's piece is the demonstration of how Polish revolutionary ideology fits into general European patterns and how it anticipated later developments in nineteenth-century Russia. Another valuable selection by Stefan Kieniewicz takes Lenin's formulation of the "revolutionary nobleman" and examines the unique role played in Eastern European thought by certain segments of the upper classes. Their influence proved to be more important in creating revolutionary legends than in accomplishing any lasting changes.

Another valuable feature of this book is its treatment of the interplay between Poland and America during the age of the democratic revolution. Those who were introduced to this rich subject by Palmer's two volumes will find further amplification here. Included are articles dealing with various crosscurrents flowing between the two countries, especially the political theory that tried to understand why one nation gained independence while the other was being swallowed up by neighbors.

The liberal character of the Polish contributors is evidenced by cryptic remarks with contemporary relevance that creep into these pages. Thus the article on Jacobin dictatorship argues that, although an authoritarian form of government was useful in mobilizing France's struggle for freedom, the excessive violence and cruelty of the Terror was self-defeating and led to the ultimate collapse of the Jacobin cause. The only article from the Polish side

that might be thought to approach an official Marxist-style rigidity is a University of Poznań professor's discussion of revolutionary consciousness as a historical problem. But even he is willing to acknowledge the importance of ideologies. If he had allowed himself to absorb the nuances of his subject in the way that characterizes the efforts of certain Western Marxists like E. P. Thompson, Eric Hobsbawm, and Albert Soboul, he might have attained the sophistication realized by his own colleagues.

In a brief review of such a varied work it is impossible to do full justice to all the fine contributions. But certainly mention must be made of Robert Foster's revisionist article on the "new" French elites in the early nineteenth century. The quotation marks indicate the author's belief that continuity with the Old Regime was a major theme of the period. Also, Istvan Deak's use of a revised chapter from his excellent recent study of Louis Kossuth and the Hungarian Revolution adds an interesting comparative perspective to Polish events. Finally, Jaroslaw Pelenski's introduction to this collection sets a provocative tone to what must have been a most stimulating historical conference.

LAWRENCE KAPLAN
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JOHN J. MCCUSKER. *Money and Exchange in Europe and America, 1600–1775: A Handbook*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1978. Pp. xi, 367. \$26.00.

What is *Jersey Light*? Hint: it is neither a beer nor a cigarette. Give up? The answer is on page 169 of John J. McCusker's estimable *Money and Exchange in Europe and America, 1600–1775*. This volume is a handbook. It is one of those rare works that seeks nothing more than to help the historian—in this case the historian who needs "to convert a sum stated in one money into its equivalent in another money" (p. 3). Actually this seemingly simple task is nothing of the sort. Reconstructing the archaic knowledge of the colonial merchant is highly challenging to the modern historian, and, as McCusker correctly observes, few of us have the patience to devote ourselves to such a reputedly "dry" subject.

McCusker's valuable handbook gives the impatient historian guidance and information concerning the moneys, the rates of exchange, and the payments networks of the Atlantic powers and their New World colonies in the seventeenth century and the eighteenth century up to the eve of the American Revolution. His chief interest is in the colonies, but the nature of the exchange system required that he pursue his subject to Europe. Consequently, the

historian of Europe will find this handbook as useful as will the colonial historian.

The heart of the handbook is a series of tables providing all the exchange-rate quotations McCusker has been able to gather in many years of searching. They are presented monthly, year by year, and include annual averages. The only European rates of exchange presented are on London, but others such as Hamburg on Amsterdam, can be computed by using the Hamburg on London and London on Amsterdam tables. Of course much was already known and published about these rates; the same cannot be said for the colonial rates of exchange. Here McCusker's dedication and archival industry have resulted in the amassing of sufficient bits and pieces of information to produce tables for all the English continental colonies, several Caribbean colonies, and a few non-English colonies. In all cases the tables are meticulously documented.

For every nation, colony, or natural group of colonies McCusker prefaces the tables with a commentary that introduces the currency system and its legislative and market history, discusses problems with the sources, and offers examples of exchange conversions to keep the reader on his toes. The author goes on to make valuable observations on his findings that go well beyond what one would expect to find in a pure handbook. The format makes for repetitiveness, but that is a problem unavoidable given the volume's consultative purpose.

Our professions should be grateful to the author and publisher for making available this useful product of painstaking research.

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DANIEL R. HEADRICK. *The Tools of Empire: Technology and European Imperialism in the Nineteenth Century*. New York: Oxford University Press. 1981. Pp. x, 221. Cloth \$14.95, paper \$6.95.

Daniel R. Headrick's book on *The Tools of Empire* adds still another dimension to the vast literature on the causes and motives of nineteenth-century European imperialism. The author did not intend to write a critique of imperialism as such or an analysis of its underlying motives. He has limited himself to the exposition of some of the key technological developments that played a role before and after the "scramble for Africa" during the latter part of the nineteenth century. His investigations of the technological innovations that affected the economic and political consolidation of European rule in Africa give us valuable insights into the making of imperialism.

Headrick explains how the perfection of steamers

and gunboats during the 1830s enabled England to establish its supremacy over India, the Near East, and Africa. He reports on quinine prophylaxis, which began in the 1840s and was improved throughout the nineteenth century. He examines the impact of modern weapons on the successful control of the African interior. The new breechloaders and further advances in the safety and performance of modern weapons enabled the colonial administrators and the military to maintain order with fewer losses. Headrick credits the vast and well-built network of the railroads constructed by the British in India with the more secure establishment of British government in that vast subcontinent.

Technological factors also had a significant impact on the expansion of trade. The groundnut and palm oil railroads in West Africa, and the copper railroads from Katanga to Rhodesia in Central and East Africa are a good example. A further milestone in the European penetration of the African interior was reached with the introduction of quinine prophylaxis. Headrick details the many futile attempts to combat the African "fevers" until a decisive breakthrough was reached in the 1830s with the extraction of quinine from cinchona. This landmark of technological progress was followed by better methods of producing quinine scientifically, and, as the author points out, its scientific production and its regulated distribution on the world markets are an interesting example of "imperial technology." Without it, he says, European colonialism in Africa could not have succeeded (p. 74). Furthermore, the ever-growing arsenal of technological tools contributed to the phenomenal increase in the tonnage of shipping from 700,000 tons in 1850 to 25,000,000 in 1910 (p. 166). Equally impressive was the political and social impact of modern railroads in the rugged African territory toward the end of the nineteenth century. Whereas formerly 13,333 African porters would have been required to carry fifty tons of freight at a distance of twenty-five to thirty kilometers per day, the new trains could carry the same amount at twenty kilometers per hour safely (p. 193).

Can the "scramble for Africa" during the last quarter of the nineteenth century be better understood by paying more attention to the technological factors involved? Has modern technology been helpful to the rural societies of the independent African states after the departure of the colonial powers? Headrick's answer is cautious. "The new technologies in the nineteenth century," he writes, "made imperialism so cheap that it reached the threshold of acceptance among the peoples and governments of Europe and led nations to become empires" (p. 206). But the "cheap victories on the imperial frontiers" and the powers that colonial governments acquired threatened the delicate balance of forces before 1914 (p. 209). Headrick raises

the question of the value of a legacy that passed on to the peoples of Africa the European fascination with machinery and innovation, . . . the true legacy of imperialism" (p. 210). It is this "true legacy," however, that is the major problem of the developing African countries today. Headrick's analysis of technological advancement in Africa is limited to the past and does not necessarily have to concern itself with the impact of the past on the present conditions in Africa. As a history Headrick's evaluation of *The Tools of Empire* will serve as a valid guide to many aspects of colonialism that have not been given the attention they deserve.

ANN BECK
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ANTHONY R. DELUCA. *Great Power Rivalry at the Turkish Straits: The Montreux Conference and Convention of 1936*. (East European Monographs, number 77.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. viii, 216. \$16.00.

The Montreux Conference of 1936, which produced an international convention that still governs passage of ships through the Turkish Straits, has generated a considerable literature, much of it by specialists in international law or relations. The virtues of Anthony R. DeLuca's book are its historical approach and its use of British Foreign Office documents.

Most of the account concerns the period from 1933 to 1936. In the revisionist climate of Europe, especially after Italy's invasion of Ethiopia and Germany's military reoccupation of the Rhineland, Turkey felt threatened. The Lausanne Convention of 1923 had demilitarized the zone of the Straits and created an international commission to supervise ship transit. Turkey wanted to fortify the Straits for defense and to control the Straits regime itself. Its proposal for treaty revision by conference, a welcome change from the Hitler-Mussolini type of revision, was accepted by all Lausanne signatories except Italy. In the Montreux sessions, Turkey pressed for sovereignty and security, Britain for freedom of passage, Russia for a special status for Black Sea states. Compromise produced a convention that gave the Black Sea states a much better position than the Lausanne agreement had. Turkey was allowed to refortify, and the commission was abolished. DeLuca follows the negotiations in some detail, principally from the British records. He has also used some published Russian materials and a generous selection of 1930s commentaries. This is not a definitive account, although on British policy it is good. Turkish views are presented from British, sometimes German, documentary sources. This is

helpful—on Numan Menemencioglu's visit to London, for instance—but hardly final.

The last chapter summarizes events of 1936 to 1946 concerning the Straits, especially alleged war-time violations of the rules and inter-Allied moves toward revising Montreux that ended in the rather stiff and inconclusive exchanges of notes by the Big Three in 1946. Although Foreign Office documents are used through 1945, this section adds less to our knowledge. Harry N. Howard's *Turkey, the Straits, and U.S. Policy* (1974) is more informative.

Occasionally DeLuca is hard to follow, owing partly to discussion of technical questions, partly to the writing, and partly to insufficient dates in both text and notes. The whole Montreux Convention is neither reproduced nor summarized; the reader needs its text at hand. Typographical errors are frequent. Finally, the preface unfortunately proclaims the "intention to prepare an extensive and exhaustive study of the geopolitical significance of the Turkish Straits within the twentieth century context of war and power politics." The book is nothing of the sort, either in time span or breadth of topic. It is a useful work on the Montreux Conference and Convention, especially on the sensitive questions of passage of warships, belligerent or peaceful, through the Straits when Turkey is neutral. It reaches sensible conclusions: that Turkey and Russia gained much by the revision; that Britain, an apparent loser, also gained much by realistic negotiation and by winning Turkish friendship; that the convention was no success for the League of Nations or for collective security; and that this peaceful revision of a treaty, although successful, offered no very useful precedent.

RODERIC H. DAVISON
George Washington University

ROBERT W. DESMOND. *Windows on the World: The Information Process in a Changing Society, 1900–1920*. Iowa City: University of Iowa Press. 1980. Pp. xii, 608. \$28.50.

The first two decades of this century put the world community rapidly, breathtakingly in touch with its various parts with an intimacy and incessancy never before dreamed of. This quantum leap in world news and communications was the origin of the global village we know today. A series of global disasters and crises, culminating in World War I, the proliferation of transoceanic cables and rapid development of radio communication, and the large-scale deployment by aggressive newspapers and wire services of foreign correspondents throughout the world combined between 1900 and 1920 to open the "windows on the world" for citizens of many nations.

Robert W. Desmond's copious description of

these factors and their interplay is distinctly journalistic, featuring an enormous array of names, dates, and details and implicitly assuming the rightness of these historical developments. He bases his work on a familiar and hopeful premise: "Current information is the key to knowledge. Knowledge is the key to understanding. Understanding is the key to a civilized way of life for mankind" (p. xi).

The world lost its innocence during those twenty years of rapid development in international news movement. After World War I, says Desmond, "there was to be no retreat to a world of simpler times and lesser issues. . . . The press and public had no choice other than to move with events" (p. 427). This is the extent of Desmond's analysis of the monumental forces at work on the world stage between 1900 and 1920.

Desmond, with a journalistic background on several U.S. metropolitan newspapers and with teaching stints at Michigan, Minnesota, Stanford, Northwestern, and California-Berkeley, devotes nearly half of this book to providing background for the reader on the pre-1900 and post-1920 developments in international news and communication surrounding his chosen twenty-year period of study.

Windows on the World is the second volume of a projected five-volume series on world information flow. The first, *The Information Process, World News Reporting to the Twentieth Century*, was published in 1978. Volume 3 will carry the story up to World War II, which is to be the subject of the fourth volume. The fifth will discuss post-1945 world news reporting. Like the first volume of this series, *Windows on the World* fails to cite sources for its myriad of facts and details. Rather than concluding with an essay on sources, Desmond merely presents an extended bibliography, composed almost exclusively of secondary, English-language sources, the bulk of which are biographies.

The absence of specific citations, even for large segments of the text, leaves fellow scholars in the bewildering position of either accepting Desmond at face value as the final word on this subject or replotting Desmond's and other sources when conducting parallel or tangential research.

Had the book tilted toward an essay or analysis, such an omission might be understandable, but as an encyclopedic compendium of historical details, it cries out for sources and underpinning. Although Desmond has obviously expended enormous time and energy on this project, by failing to discuss or reveal his sources he fails to assist further research on this all-important subject.

RICHARD A. SCHWARZLOSE
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TIMOTHY P. IRELAND. *Creating the Entangling Alliance: The Origins of the North Atlantic Treaty Organization*. (Contributions in Political Science, number 50.)

Westport, Conn.: Greenwood Press. 1981. Pp. x, 245. \$27.50.

TIMOTHY P. IRELAND. *Creating the Entangling Alliance*: is a straightforward account of the forces that brought the idea of such a treaty into being, shaped its structure, and finally shaped American agreement to an integrated command and the placement of a sizable contingent of troops in Europe in 1950–51. In tracing these developments he has clearly articulated the thesis that the ultimate form of NATO emerged not only from America's concern with establishing a balance of power in Europe against the USSR—by reinvigorating the European economy and its military power—but also from Europe's concern, and especially that of the French, to involve America fully as a weight in the internal European balance of power. The result of this tug of war over the meaning of NATO provides the "entanglement" of Ireland's title—the involvement of American power in European politics in ways that American policy makers had not originally intended.

Ireland shows how each of the decision points—the Marshall Plan, the Brussels Pact, the formulation of NATO itself, the debate in the United States Senate, and the implementation of the treaty during the Korean War—forced the United States to consider Germany not just as a frontline state and a source of economic strength in the Cold War but also as a part of the Western European balance of power. The strength of the book lies in this careful examination of how the Allies had to come to terms with each other's concerns and how NATO ultimately emerged from their mutual accommodation as much as it did from the exigencies of the Cold War itself.

Ireland's book is a competent monograph, tightly focused on its thesis, but of limited utility. Anyone who has a knowledge of the literature of the Cold War will find little that is new either in terms of documentation or interpretation, because in large part the book is based on well-known printed sources, either secondary works or the *Foreign Relations of the United States* series. Furthermore, the book has a somewhat bloodless quality: there is no sense of the complexities of politics at that time or of the passions aroused on both sides of the Atlantic among either the public or the decision makers themselves. If the issue of "entanglement" was as important as the author claims, one would like to see some greatness in the struggle. Finally, conceptually the author does not come to grips with some of his key terms or some of the problems raised by his book. Thus, the idea of entanglement, crucial as it is to his argument, is left unclear. To what extent and in what ways was America entangled in European politics as a result of NATO? And what is its

significance? At no place is this issue clearly faced. And the term "balance of power," as usual, is so loosely used as to be almost meaningless.

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MICHAEL MANDELBAUM. *The Nuclear Revolution: International Politics before and after Hiroshima*. New York: Cambridge University Press. 1981. Pp. xi, 283. Cloth \$29.95, paper \$8.95.

These essays address "a single theme: How have nuclear weapons affected international politics?" (p. ix). Each begins with Thucydides to emphasize the main point of chapter 1: "There is still a system of independent yet interdependent states of the sort that has existed since antiquity." The essays that follow compare nuclear with chemical and biological weapons, the nuclear and Vienna balance-of-power systems, the nuclear arms race with the Anglo-German naval race and with tariff competitions, NATO with earlier alliances, and the American nuclear-age presidents with Pericles rather than with the president of presidents. "The Bomb, Dread, and Eternity" might have been replaced by a narrative summary of Michael Mandelbaum's *The Nuclear Question: The United States and Nuclear Weapons, 1946–1976* (1979).

Most of us (Mandelbaum's survey of our fears, ignorance, confusion, and apathy suggests) need more guidance through those apocalyptic events and nonevents that have led us from what Giulio Douhet saw as the mechanized "disintegration of nations" to deterrence through mutually assured destruction. The resulting military and political crises have covered more than three political generations. That their more obvious turning points—the Marne, Hitler's successes in France and Russia, Pearl Harbor, Hiroshima, the Cuban missile crisis, Tet—have obscured other changes is understandable. So this book is a series of appendixes to *The Nuclear Question* and "International Stability and Nuclear Order" in David C. Gompert, editor, *Nuclear Weapons and World Politics* (1977).

Occasionally, in his wide-ranging historical forays, Mandelbaum was trapped by the wild anachronists and false analogists. Occasionally he trapped himself with statements like "the nuclear system is bipolar" (p. 73), or "[the] nuclear arms race has been the only one in history to provide a clear standard of military adequacy," however, "the United States by affluence, the Soviet Union by extreme defensiveness" have exceeded it (p. 146). So much for France, China, or Israel, for whom "adequate," as Mandelbaum notes elsewhere, is whatever it takes to decapitate their most immediate enemy. Mandelbaum has much to say. He says it well, and what he says is based on very wide reading. We can hope that he

will eventually put it all together in that single analytical narrative promised by the subtitle of his Gompert article, "The First Nuclear Regime." He just might write its history from the Marne (he has ten index references to the Great War to two for World War II) to its end in the 1970s and 1980s.

THEODORE ROPP
Duke University

RICHARD NED LEBOW. *Between Peace and War: The Nature of International Crisis*. Baltimore: Johns Hopkins University Press. 1981. Pp. xi, 350. \$24.50.

EDWARD MCWHINNEY. *Conflict and Compromise: International Law and World Order in a Revolutionary Age*. New York: Holmes and Meier. 1981. Pp. 160. Cloth \$17.50, paper \$8.50.

Avoidance of war is acknowledged to be a—perhaps the—critical contemporary problem. From different perspectives and at different levels, these works analyze this issue and offer suggestions for its resolution. Both combine historical with nonhistorical methods in manners that will challenge historians to reconsider the potential utility of interdisciplinary approaches. Both respond to Feuerbach's admonition that we should not merely understand but should also change history. They consequently force the historian both to formulate his craft more consciously and ponder its role in the contemporary world.

Richard Ned Lebow focuses on the problem of international crises. He offers an original and jargon-free treatment of an area that has received much attention from diplomatic historians and political scientists. Combining comparative historical methods with a variety of social science theories, he uses history as a laboratory to derive patterns that are then tested against twenty-seven historical cases. In contrast with most historians who regard underlying causes as the more significant, Lebow argues that immediate causes—that is, crises—can have an important, even decisive effect in determining whether or not war breaks out and in either increasing or decreasing the sources of subsequent conflict if war is avoided. The degree to which crises affect events depends on the type of crisis. Those that are mere excuses for war or that result from other crises usually end in war, are therefore caused by long-term factors, and accordingly have little independent influence on the course of international relations. Those crises in which one side pursues diplomatic gains at the risk of war are, however, affected by decisions taken during the crisis and are thus more critical. When such crises eventuated in war, it was, according to Lebow, generally due to miscalculation caused by statesmen's mistaken perceptions of internal and external threats that were

then rationalized by rejection of contradictory information. This complex of fearful aggressiveness and perceptual bias makes Lebow skeptical of the efficacy of deterrence as a strategy of conflict avoidance. Although crisis management is clearly crucial in such situations, Lebow suggests that techniques are less important than the ability to correct misjudgments through open-minded decision making, a cohesive elite, and a commitment to peace. But since crises are thus determined largely by factors over which leaders have little control, they can affect the outcome of crises only by precrisis actions. Lebow accordingly concludes that "in this sense we have come full circle by finding the most important attributes of the immediate causes of war to be themselves a function of underlying causes, albeit of a domestic nature" (pp. 335–36).

Edward McWhinney would seem to agree in his search for a useful role for international law in establishing order in a world of revolutionary change. His succinct and accessible overview places the problem in the context of international relations since World War II. The "one world" ideal of the United Nations was displaced by Cold War bipolarity that first politicized the United Nations and then tended to disregard the organization when superpower detente developed through bilateral agreements. Detente nonetheless represented an emergent international order based on mutual acceptance of territorial frontiers and reinforced by agreements on arms, science, and technology and by gradual reduction of disagreements over conceptions of international law as represented by the United Nations and World Court. Meanwhile decolonization fostered a second division between North and South that revived the United Nations as the primary arena for Third World efforts to create a more just world economic order. The recent breakdown of detente on the one hand and the Third World unanimity on the other (into rich "New Third World" and poor "New Fourth World") has produced greater dissonance and pluralism. McWhinney thus urges a more flexible approach to international law that acknowledges that our legal conceptions are culturally bound rather than universal verities and that emphasizes pragmatic, step-by-step problem solving rather than abstract, general principles. McWhinney argues that the record of agreements over arms, science, and technology demonstrates the virtues of attacking large problems piecemeal and thereby avoiding ideological obstacles. But his study likewise reveals the limits of international law. International relations are ultimately determined by power, as Acheson asserted after the Cuban missile crisis: "The survival of states is not a matter of law" (p. 28). Thus, however detente improves communication, it persists only as long as superpower interests remain unthreatened.

Furthermore, superpower detente presupposes preservation of the postwar status quo, which precludes the economic revisionism advocated by the Third World. In short, international law is as politicized as ever.

These works inspire some hope but considerable concern about the prospects for avoiding international conflict. Lebow provides insights into the nature of international crises and some suggestions for their avoidance. McWhinney is encouraging on the efforts of international law to adjust to a changing world and to reduce some underlying sources of conflict through pragmatic and patient approaches not unlike those advocated by Lebow. But Lebow concludes that crises are generally caused by factors that are not only profound but also particular and thus difficult to alter or predict. And McWhinney demonstrates that international law reflects the prevailing political, economic, and cultural realities. The problem of conflict consequently remains trapped in what William James called the thicket of reality, that is, the complexities of historical circumstances. As a historian, one is reassured thereby that history remains a valid, perhaps necessary approach to understanding international relations. But as a citizen of a world threatened by destruction, one is disquieted by the realization of how little can be done to alleviate the causes of war.

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W. M. SCAMMELL. *The International Economy since 1945*. New York: St. Martin's Press. 1980. Pp. x, 226. \$27.50.

W. M. Scammell's volume is divided into three sections covering the immediate postwar period (1945–55), the prosperous years (1955–64), and the "crisis" ones (1964 to the present). At the end of World War II, the United States was the undisputed principal economic power. For Scammell, the foundations of the postwar world economy were laid with the Bretton Woods agreement in 1944, providing for the International Monetary Fund and the World Bank; the former was of key significance and its history provides the backbone of this book.

Scammell takes us through the dollar shortages of the first postwar decade. He shows how Europe "stumbled to recovery and external balance," considering in turn the Anglo-American Loan Agreement, the Truman Doctrine, the Marshall Plan, Cold War issues, the Korean War, and the European Payments Union. He tells of the rationale for and provisions of the GATT (General Agreement on Tariffs and Trade). The greatest weakness in Scammell's coverage of 1945–55 lies in his treatment of the Third World; he used 1955–60 figures (p. 72)!

The text is competent on the rationale for foreign aid in the early 1950s; it shortchanges, however, the persistent U.S. view that private capital rather than official assistance should whenever possible be employed in aid of less-developed countries. Moreover, developments in India, Saudi Arabia, and Brazil in 1945–55, for example, are never mentioned, while individual European nations do receive specialized attention.

During 1955–64 the Cold War dominated the world economy. Scammell writes of the origins and development of the European Economic Community and the emergence of the dollar glut and of Eurocurrency markets. He makes gratuitous comments about "faceless" multinational corporations, whose loyalties are "to nothing save their own profit and loss account" (p. 106). He is confused in comparing U.S. exports (flow figures) with U.S. foreign direct investments (stock figures). Indeed, his chapter on multinational corporations adds nothing new. The chapter covers more than the 1955–64 years, but Scammell fails to realize the significance of the relative decline of direct foreign investment in extractive industries as developing countries became owners of their own natural resources. He omits data on the substantial growth in recent years of non-U.S. multinational corporations. Although he is familiar with the theoretical literature on trade and payments, on multinational enterprise he neglects key books and is unaware of the recent work on internalization. By contrast, Scammell is at his best on the high summer of the Bretton Woods payments system, 1959–71.

In his 1964–80 section Scammell includes data on UNCTAD (the United Nations Conference on Trade and Development), GATT, the North-South dialogue, as well as details on the Nixon measures of 1971, floating exchange rates, and the recycling of OPEC surpluses. The book has, however, no adequate explanation for the rise in significance of Japan in the world economy, nothing on the law of the sea and its international economic implications, and in this part, as earlier, international migration is neglected. Also given short shrift are matters of technology transfer (the spread of advanced industries) and changes in productivity levels in recent times. Scammell considers oil more in terms of financial resource transfers than as an international energy issue. The book, in short, has a heavy emphasis on payments questions. Scammell concludes that since the 1960s the United States has no longer been the economic leader of the West, but no country has replaced it; the power structure has become one of groups rather than entities. Each reader must decide whether Scammell proves his point.

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EDWARD W. CONSTANT II. *The Origins of the Turbojet Revolution*. (Johns Hopkins Studies in the History of Technology, new series, number 5.) Baltimore: Johns Hopkins University Press. 1980. Pp. xiv, 311. \$22.50.

Most of us enjoy and sometimes curse such technological accomplishments as the jet engine and aircraft without presuming to understand the technology or its sources and without reflecting on the social, economic, political, and military implications of that technology. Edward W. Constant II, in *The Origins of the Turbojet Revolution*, traces turbine development over 200 years and considers its impact on our world. In his search for the beginnings of the turbojet engine-aircraft system, Constant developed a model that may be useful to others in their examinations of technological developments.

The key element in the author's model is the "presumptive anomaly": new technology emerges not because of a failure in the conventional technological system but because an individual or group of individuals assume from scientifically derived information that "under some future conditions the conventional system will fail (or function badly) or that a radically different system will do a much better job" (p. 15). A problem or anomaly is presumed to exist—thus his concept of "presumptive anomaly." Constant notes that the turbojet revolution "depended upon prior technology, upon a two-centuries-long evolution of turbine systems" (which he summarizes), but that the turbojet itself was "a simple, linear extrapolation of no prior technology. The turbojet is a holistic system with novel properties no predecessor system ever contemplated exhibiting. The turbojet would overthrow and very nearly extinguish the technological tradition founded upon piston engines and propellers" (p. 241). Constant demonstrates that the men who created the turbojet revolution—those who perceived the presumptive anomaly—were from outside the traditional aeroengine community. Problems with propeller compressibility and airframe limitations led the outsiders to the presumption of an anomaly, first in scientific theory and then in actual practice. Within the context of his model, Constant successfully accounts for the emergence of the turbojet and in so doing makes an important contribution to the history of technology.

But as significant as this book will be to our studies, it is severely flawed by awkward and often tortuous use of the English language. Constant has tried much too hard to sound academic and intellectual. A ruthless editor would have translated such cumbersome phrases as "the hierarchical decomposition of the overall design problem into more manageable subproblems" into briefer, more concise words. Serious investigators will not be stopped

by the prose, but many casual readers will not complete their journey through Constant's book and benefit from his insights into the origins of the turbojet revolution and the potential significance of the presumptive anomaly to the understanding of technological developments.

EDWARD C. EZELL

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LOYD S. SWENSON, JR. *Genesis of Relativity: Einstein in Context*. (Studies in the History of Science, number 5.) New York: Burt Franklin. 1979. Pp. xv, 266. \$21.00.

Books come in almost all sizes and lengths. Usually, the length of a book can best be determined by the principal theses and goals of the writer. In terms of these criteria many books will be found to be too long, many too short, and a few will be found to be admirably suited to the author's purpose.

Lloyd S. Swenson, Jr., has set for himself a difficult task, that of writing a "contextual history" of one of the dominant figures in twentieth-century physics, Albert Einstein, and of his theories of relativity. It is to be an intellectual history, covering the period from about 1870 to about 1920, and placing modern science directly in the main currents of the history of ideas. The work is about the right length to make clear the experimental side of relativity, much too short to clarify the theoretical side, and too long to deal with the multitudinous details of the contemporary scene.

Much of this problem stems from the fact that the author has written at several levels for several distinct audiences: for the man in the street, for the scientist, and for the specialist. This has resulted in a work where the experimental side is explained in good detail for the general reader; where the theoretical side (dealing with the properties of invariance and covariance, of vectors and tensors, of vector potentials, of complementarity, and so on) is tacitly understood by the specialist, but not by the man in the street; and where the evanescent contemporary scene is clear to the scientist but is only a confusing blur to other readers. To have expanded the theoretical side would have resulted in a much longer and a much more difficult work; to have eliminated the detail would have left the reader without a sense of the vital and burgeoning nature of science during this historically rich period.

Another problem arises from the fact that much of the contextual history is achieved by merely juxtaposing related ideas, leaving to the reader the task of making explicit the relations among them. The net effect, however, is to deprive the reader of the author's interpretation of the causal evolution of these ideas.

After the excellence of the author's earlier work in the same area, *The Aethereal Aether* (1972), it is a disappointment to find the work under review to have been of considerably lesser quality.

W. JAMES KING
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E. STUART KIRBY. *Russian Studies of Japan: An Exploratory Survey*. New York: St. Martin's Press. 1981. Pp. xv, 226. \$25.00.

Russian studies of Japan constitute an object worthy of attention in the West, if not always for their scholarship, then for the light they shed on Soviet perceptions. According to the jacket blurb, this is "the first book in any language to give a full treatment of this subject." The claim is questionable. Moreover, the author's assertion that "there is no work of the kind in the language of Japan itself (pending a translation of this volume)" (p. ix) does not take into account excellent studies by Takano Akira (1968) and Teruoka Shūzō (1980).

"Full treatment" consists of a smorgasbord of sketches of Russian and Soviet authors who have written about Japanese history, economics, literature, linguistics, philosophy, law, religion, and the arts. A bibliography of selected works by selected authors (listed only in English) closes this slender volume.

The author allows that "this work is heavily indebted to the industry of Soviet bibliographers." Indeed, it appears to be based largely on a two-volume bibliography of Japan published in Moscow in 1960 and 1965 and a biobibliographical dictionary of Soviet Orientalists edited by Sofia Davidovna Miliband (1975). There is little information on authors after 1974 (Miliband's cut-off date) and virtually nothing about authors—however respected or influential—who are not listed in Miliband.

This book suffers from an acute shortage of analysis. Commentary, when present, is generally superficial. Avenues of potentially fruitful inquiry are by-passed. Questions are not asked about how Japan specialists are recruited, trained, employed, and retired. The bureaucratic and personal networks between research institutes, universities, government ministries, and party organs are not touched upon. One looks in vain for discussion of how Soviet Japanologists obtain research materials, cope with working conditions, organize and administer research, and secure access to Japan. The intellectual and sociological environment molding Japanese studies in the USSR receives no systematic scrutiny.

Biographical and bibliographical information on Soviet Japanology would in itself be a valuable contribution to knowledge. This volume falters even

on that score. Omissions abound. There is no discussion of the late Nikolai Aleksandrovich Nevskii, a brilliant scholar of Japan and Okinawa, who is internationally recognized as a Russian who helped the Japanese understand themselves. A whole new generation of specialists writing about Japanese politics (Mikhail Nosov, Konstantin Sarkisov, Georgii Kunadze, and others) are not mentioned. Long-established authors such as Elena Leontieva, Georgii Komarovskiy, and Igor Derzhavin do not appear, perhaps because they happen to have no entry in Miliband. The most incomprehensible omission is Ivan Ivanovich Kovalenko, an authoritative commentator on Japanese domestic and foreign policies, editor-in-chief of the yearbook *Iaponiia*, and who, as head of the Japan Section of the CPSU Central Committee's International Department, is probably the single most influential Japan specialist in the Soviet Union.

Russian Studies of Japan contains too many errors to list here. The style is jumbled and the contents repetitious. In one instance, the same person is discussed in two nearly identical paragraphs on pages 18–19 and 125. But the greatest disappointment in this volume is an insensitivity to analytic differences among Soviet authors. An awareness of such differences could help correct a common stereotype—apparently shared by the author—that Japanese studies in the USSR are an inflexible monolith.

JOHN J. STEPHAN
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ALOYSIUS BALAWYDER. *The Maple Leaf and the White Eagle: Canadian-Polish Relations, 1918–1978*. (East European Monographs, number 66.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1980. Pp. viii, 300. \$20.00.

Aloysius Balawyder's work is an effort to document Canadian-Polish relations from the end of World War I to the present. The focal point of these relations is the immigration of Poles to Canada. In the preface the author defines the purpose of the monograph: to demonstrate "how the relations between Canada and Poland were largely determined by functional pragmatism," to examine "the Polish community in Canada as a basis for Canadian-Polish relations," and to illustrate "the importance of democratic values in Canadian-Polish relations."

The book traces the origin of Polish immigration to Canada and its impact on Canadian immigration policies and in turn how Polish immigration was affected by Canadian policies. These policies were largely shaped by British traditions, values, and prejudices that favored British and north European

immigrants over Slavs. The policies also reflected Canada's need for a labor force and its overall foreign policy. Poland lacked a defined emigration policy. Most of the emigrants left Poland in search of better economic opportunities. In the interwar period, Poland tried to assist the emigrants by investigating the living conditions in the countries of destination and by helping them preserve their native language, traditions, and customs. By maintaining close ties with Polonia, Poland counted on its support, both financial and moral. This was especially evident during both world wars when efforts were made to recruit Poles in Canada and the United States for Polish forces.

The post-World War II immigration consisted mostly of educated and skilled Poles who on the whole opposed the new political system in Poland. Simultaneously they retained strong sentimental ties with the motherland. Polish-Canadian relations at this time were shaped by the prevailing East-West relations and by Canada's detention of the Polish art treasures. The emergence of the Gomulka government, the post-Stalin thaw, the development of detente, and the return of the Polish art treasures gradually led to improved relations between the two governments and between Poland and the Polish-Canadian community.

Although the title might lead one to conclude that the book deals primarily with the foreign policy of two nations, it is a valuable study of ethnic history, tracing the Polish-Canadian community from its origin through its development and growth.

The book is well organized and compact. Each chapter contains a short summation, and the conclusion gives an overview of the whole monograph. The work is broad in scope and the material is well documented and footnoted. The author availed himself extensively of original source materials located both in Canada and in Poland and supplemented this material with personal interviews. Balawyder's work fills a gap in the documentation of Canadian-Polish relations and Polish ethnic history. It is hoped that this pioneer work will stimulate further research and lead to more detailed publications documenting Canadian-Polish foreign relations and the history of the Polish ethnic group in Canada.

ZOFIA SYWAK

Rhode Island Historical Records Survey

ANCIENT

PATRICK F. O'MARA. *The Palermo Stone and the Archaic Kings of Egypt*. (Studies in the Structural Archaeology of Ancient Egypt.) La Canada, Calif.: Paulette. 1979. Pp. xvi, 208.

PATRICK F. O'MARA. *The Chronology of the Palermo and Turin Canons*. (Studies in the Structural Archaeology of Ancient Egypt, number 2.) La Canada, Calif.: Paulette. 1980. Pp. vii, 160. \$20.00.

That the Egyptians had excelled all other peoples in the accuracy of their recording of the past was a cliché among classical authors. Egyptian records themselves tend to confirm this; but surprisingly few king lists or *Annales Maximi* have survived. We have but one true king list, the Turin Canon (all others are only cultic assemblages of royal ancestors), and a single edition of annals properly so called, the Palermo Stone. The term "annals" in Egyptian later came to be applied to mythological works as well, but that is another matter. A fresh examination of both these documents was, and in spite of Patrick F. O'Mara's work, remains long overdue.

The Palermo Stone, being but a small fraction of the original document, has consistently defied reconstruction; all estimates as to its pristine size turn out to be rough approximations. O'Mara too meets an impasse. Having taken sixty pages to discover that precision measurement leads to a dead end, and having enunciated the chastening thought that any future attempts must proceed upon the basis of the Egyptian cubit, he nevertheless refuses to surrender and launches into other avenues that seem to promise solutions. This is a pity; he should have surrendered. Now the assumptions and presuppositions come thick and fast, almost all of them wholly unwarranted. The author assumes symmetry and balance in the arrangement of the stone; yet he has no idea what the ends of the stone looked like, whether divine or royal figures stood at certain points, foreshortening rows of rectangles, nor whether the rectangles themselves varied in width, and in some rows they do, rendering any final conclusion impossible. The first three dynasties are allowed 382 to 383 rectangles, but this is fewer than we would have expected from Manetho. Therefore later lists must be guilty of having invented and duplicated names. The Cairo fragments, of course, do not support him, and so he is at pains to dismiss them as fakes. (A rebuttal here would take too long; but I can assure him that, had he studied the fragments at first hand, he never would have reached this conclusion.) Next, on the basis of the alleged identity of the building in PD IV, no. 44-45 with that mentioned in Palermo *recto* iii, 6-8, this hitherto unidentified reign is assigned to $\text{K}^{\text{a}}\text{a}$, rather than Den (as is normal). O'Mara is here guilty of sleight of hand; the two buildings are not the same, the former being $k^3w-ntrw$, the latter $swwt-ntrw$. The difference in the writing of the signs is quite clear.

If *recto* iii is assigned to $\text{K}^{\text{a}}\text{a}$, there is now not

enough room on the stone for his seven canonical predecessors; the author concludes that the first four of these (Menes to Wadji) never were there. The ancients who composed Ur-Palermo knew of the sequence Den-Enedjib-Semempses-Ka'a from their knowledge of the pottery dockets of the Step Pyramid enclosure, but they erroneously placed them at the beginning of the dynasty. How could these early chroniclers make such a botch of their early history? They had forgotten it, they had no means of knowing it, their methodology was faulty, their "architectonic minds" relied on symbolic constructs, and so forth.

By this time anyone who has even a nodding acquaintance with Egyptian historiography may well have given up. Errors and implausible reconstructions abound. Menes is gloatingly disposed of as a mistaken reading of *Smtj*; the biennial cattle count is a false retrojection from the fifth dynasty; O'Mara mistranslates *msi* as "to fashion," misunderstands formulaic expressions, and daringly renders archaic hieroglyphs. Most serious of all, he shows no awareness of the historiographical genre of "annals" (*gnwt*), references to which date from the Old Kingdom on. The scribes of the Palermo Stone had no need to consult the haphazard fragments of graffiti from the Djoser complex: they had access to the pristine record of the royal annals!

O'Mara's use of Manetho's *Aegyptiaca* is a curious mixture of inconsistency. In his unorthodox reconstruction of the Palermo Stone he at one and the same time finds Manetho an unholy composite of several mutually exclusive sources, and yet when it suits him he accepts the lengths of reign as gospel! One may be shocked to learn that numbers are to be considered more durable in tradition than names; yet in his reconstruction of the canon he does not shrink from keeping names and rejecting numbers. One may well wonder how the latter-day Manetho preserved correct figures on the whole, when we are told that even the Old Kingdom Egyptians could not have remembered lengths of reign with accuracy. How can the author hope that we will take seriously his prestidigitation with numbers when he ignores the one major factor that has warped Manetho's figures, that is, the tendentious use of the *Aegyptiaca* in the "chronographic" games played by those involved in the Judeo-pagan (later Christian) polemic?

I regret to say that I have only just begun to single out the shortcomings of these two volumes. A new study of Palermo and Turin awaits the writing, but when it appears it will be found first and foremost to be a form-critical analysis, not an exercise in number juggling.

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VINCENT GABRIELSEN. *Remuneration of State Officials in Fourth Century B.C. Athens*. (Odense University Classical Studies, number 11.) Odense, Denmark: Odense University Press. 1981. Pp. 165.

In *Politics* 1317b, with contemporary Athens in mind, Aristotle characterized pay (*misthos*) for all magistrates as one of the hallmarks of a democratic constitution. In *Athenaion Politeia* 62.2, however, he lists only a handful of paid magistrates in addition to the Council of Five Hundred. His silence about pay for the hundreds of other known officials was regarded as crucial by M. H. Hansen, who argued (in *Symbolae Osloenses*, 54 [1979]: 5–22) that no fourth-century magistrates, except the councilors, were paid anything more than small daily allowances for food (*trophe*, *sitesis*). The fifth-century policy of pay for all officials had been abolished forever in the oligarchic revolutions of 411 and 403.

Vincent Gabrielsen's monograph sets out to refute Hansen's claim and to argue for continuity from the fifth century until at least the 320s in the policy of pay for all or most magistrates. He convincingly demonstrates that the oligarchic regimes of 411 and 403 probably brought only temporary cessations of pay for magistrates. His reconstruction of the attitudes of Athenian democrats toward remuneration for officials both during and after these crises is plausible, although in discussing the revolution of the Four Hundred he badly mistranslates *politeusomen sophronesteron* (Thuc. 8.53.3) as "a wiser form of democracy," and his bibliography is thirty years out of date.

Gabrielsen argues that the short list of magistrates in Aristotle, *AP* 62.2 is not significant since (1) there are many other omissions of known fourth-century magistrates from *AP* 42–69 and (2) the *AP* is merely a rough, incomplete collection of notes never intended for publication and not written by Aristotle anyway. Here he also tries to prove the synonymy of the terms *misthos*, *trophe*, and *sitesis* in *AP* 62.2.

The most original part of the work, chapter 5, is a "structural analysis" of the socioeconomic status of Athenian magistracies. From a close reading of the Attic orators Gabrielsen infers that the majority of officers appointed by lot were men from the lower social and economic strata. Since such people depended for their livelihood on income-producing occupations, most could not have abandoned their jobs for a whole year to serve in public office unless they received remuneration. He further argues that if they were unpaid, many Athenian magistracies would have resembled liturgies and we ought to encounter as officeholders many of the same people who also served as *choregoi*, *trierarchoi*, and the like, and who paid the *eisphora*-tax. The absence of any reference to *archai* in many passages where speakers

boast of their liturgies and *eisphorai* payments indicates to Gabrielsen that magistracies did not impose a financial burden on their incumbents; hence they were salaried.

These are interesting, if inconclusive, new twists that this competent work now adds to the majority view about pay for fourth-century Athenian officers. Unfortunately the book is disfigured by over a hundred misprints and grammatical errors.

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RONALD P. LEGON. *Megara: The Political History of a Greek City-State to 336 B.C.* Ithaca: Cornell University Press. 1981. Pp. 324. \$25.00.

The historian of Megara must face the reality that there is little archaeological, epigraphical, or numismatic evidence, and the literary sources are meager, widely scattered, and, for the most part, late and of questionable reliability. In attempting to use this limited information, Ronald P. Legon has undertaken a formidable task.

After an introductory chapter on the geography of the Megarid, five chapters cover the history of the city down to the Persian Wars; four chapters, including one entitled, "The Megarian Decree," deal with the fifth century; and a final chapter carries the story down to 336 B.C. Megara played a different role in each of these periods. Nothing can be said of the period prior to the middle of the eighth century, but the formation of the Megarian polis about that time led to trouble with Corinth over control of the western Megarid. By the early seventh century this territory had been lost to Corinth, but for the next two hundred years Megara was in the vanguard of Greek development, economically and politically. As the Megarians pioneered in colonization and commerce, their purely agrarian economy became more dependent on imports. Political troubles accompanied economic change with the quick rise and fall of the tyrant Theagenes and the so-called "unbridled Democracy," before the establishment of a narrow oligarchy. There was an ongoing struggle with Athens for control of Salamis, but the loss of this island was followed by nearly a century of peace and prosperity. By 510 B.C., however, Megara had become a member of the Peloponnesian League and thereafter was more follower than leader. Caught between Athens and Sparta in the fifth century, Megara's very survival depended on one or the other of the major powers. For the most part the Megarians found greater security in alliance with Sparta but paid a heavy price and suffered severely in the Peloponnesian War. In the fourth century they found a way to remain aloof from the wars that

raged and as a result recovered quickly and enjoyed considerable prosperity.

The main lines of Megarian history stand out clearly enough, and in pointing them out the author has done a creditable job. Yet the volume is less a history of Megara than an extended discussion of our inadequate sources for Megarian history. "This book," he tells us, "would have been substantially shorter if we had had more information, for many points must be argued at length on the basis of extremely fragmentary evidence—sometimes even the merest inference of Megarian involvement in larger events" (p. 11). Given the nature of the evidence, the attempt to find a cause-and-effect relationship is seriously overdone, and too frequently problems in the sources are resolved on nothing more substantial than what the author believes, is inclined to believe, or prefers to believe. Some of these beliefs have merit; others, particularly for the early period, are supported neither by evidence nor logic.

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MICHAEL GRANT. *The Etruscans.* New York: Charles Scribner's Sons. 1980. Pp. xv, 317. \$17.50.

The Etruscans continue to exercise a fascination for scholar and layman alike, a tendency that goes back to the days of classical antiquity. Even the Roman emperor Claudius, who was for a time married to an Etruscan, produced a long treatise about them, and was subsequently honored by the erection of a statue at the once great Etruscan city of Caere. This book is however lost, epitomizing the scanty nature of our written sources on the Etruscans. Despite their importance for the history of Mediterranean civilization between the eighth and third centuries B.C., our knowledge of their history is astonishingly fragmentary—mainly because they have left us remarkably little written material.

Fortunately, the archaeological record is infinitely richer, and, as a result of work over the last two centuries, Etruscan cemeteries and sanctuaries have yielded a breath-taking range of magnificent artifacts. More recently, there has also been an encouraging trend toward the excavation of settlement sites, so that an altogether more balanced picture of the history and archaeology of the period is beginning to emerge.

It is this wealth of new information that has prompted a number of new studies of the Etruscans. There are those that are unashamedly "popular" but sometimes shoddily researched and unhappily slanted, while others are exclusively "academic" and, in consequence, all too often deeply obscure.

Michael Grant has aimed for the middle ground and has undoubtedly succeeded brilliantly, for this is a book that is both scholarly and well informed but, at the same time, beautifully written and completely lucid—a notable achievement in these days of jargon-ridden, generally incomprehensible, archaeological studies.

Grant divides his volume into two parts. One deals with the evolution of the Etruscan city-states and includes one of the most sensible discussions of recent years about the origins of the Etruscans; the other is a sensitive and helpful introduction to the history and archaeology of the leading cities and their territories. Most striking is his judicious blend of history and myth; sometimes less satisfactory is his use of the archaeological evidence. To summarize the Bronze Age "Apennine culture" as a "civilisation of a seminomadic, pastoral kind" (p. 9), really will not do, just as one doubts the *ubiquity* of his model for Etruscan urbanization: namely, that it involved the coalescing of several adjacent villages into a single settlement nucleus. There are also other occasional lapses, among them a curiously inexact description of the Tomb of the Bulls at Tarquinia. But these are small points, for the book as a whole provides an excellent, well-illustrated and—dare one say it—a gripping read for the layman; an admirable and well-referenced introduction for the student; and not a few points to intrigue the specialist in Etruscan studies.

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CLAUDE NICOLET. *Rome et la conquête du monde méditerranéen: 264–27 avant J.-C.* Volume 1, *Les structures de l'Italie romaine.* (Nouvelle Clio. L'Histoire et Ses Problèmes, number 8.) 2d ed., revised. Paris: Presses Universitaires de France. 1979. Pp. 460.

In its series, both chronological and bibliographical, this book follows the splendidly original work of J. Heurgon, which brought the history of Rome and Italy down to the year 264 B.C. Claude Nicolet takes up the story in that year with only a passing glance at the earlier years assigned to him. As the title of this second and revised edition suggests, the work is not strictly speaking a history at all. Rather, after presenting over sixty pages of useful bibliography, Nicolet surveys, in this order, the geography and demography of Italy, its agricultural economy, its attendant problems owed to continuous conscription of smallholders, and its industry and trade. These four chapters are the basis for the kind of historical studies we have come to expect from Nicolet, who has earned our thanks especially for research on the Roman money class and on public finances. Accordingly we are again indebted to him for the next two chapters, social structures and

relations, and state finances, where he deftly encapsulates the latest fruits of research peculiar to him and, to a degree, to his co-nationals.

In the seventh chapter Nicolet finally turns to historical narrative with a focus on the problem of Roman relations with its elaborate network of confederates and its outcome in the Social (Marsic) War. This narrative leads him next to a discussion of the Roman army, a subject already touched upon in earlier chapters when discussing the agrarian question and the price of constant warfare. He has deferred discussion of the army to that point where he needs to explain the army's direct involvement in civilian politics, which marked the last century of Republican government. Thereupon he can revert to what is typical format, if not typical history of the period, with his last four chapters: the people grouped in the institutions of the public assemblies; the ruling class grouped in the institution of the senate; magistrates, their powers, and their limitations; and, last, a disappointing summary of traditional elements endemic at the death of Republican government.

There is much to be learned from Nicolet's book if the reader is already reasonably grounded in the history of the period emphasized (from the Gracchi to Caesar). Naturally Nicolet is at his best in explaining matters that he himself has deeply researched. In this case, one is disappointed when he acknowledges the lack of social and economic statistical data only because such data are grist for his mill. In spite of the technical nature of the best parts of this work, Nicolet writes with clarity, for he has eschewed the jargon, not to say the sloganeering through which history has been recently supplanted by certain theoretical presumptions in some ranks of this new school.

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GRAHAM WEBSTER. *The Roman Invasion of Britain.* Totowa, N.J.: Barnes and Noble. 1980. Pp. 224. \$14.00.

The first stage in the Roman conquest of Britain took place under the governorship of Aulus Plautius between A.D. 43 and 47. Into a Britain still largely barbarian, but softened by a constant stream of luxury goods emanating from the classical world of Rome, the legions quickly thrust, meeting their first serious resistance at the River Medway several days after landing. At the Thames they waited for their emperor Claudius to join them before the short march to receive the surrender of the native capital at Camulodunum (Colchester). Native resistance collapsed, allowing the legions to fan out across the lowlands of Britain to establish their first frontier—marked by the lime road known as the Fosse

Way—which straddled the island from the region of Exeter to Lincoln. The first stage of the invasion was complete. This in brief outline is the story of Graham Webster's *The Roman Invasion of Britain*.

In 1965 Webster, in collaboration with Donald Dudley, wrote the *Roman Conquest of Britain* covering the period A.D. 43–57. Such is the pace and intensity of archaeological research in Britain that only fifteen years later, not only was the subject too extensive to treat in a single volume but also the evidence had so dramatically increased that two totally new books could be written instead. The second we eagerly await.

The Roman Invasion of Britain is that rare kind of book suitable for specialist and general reader alike. It is written with the authority of one who has carried out much of his original research in the field and with a verve and energy that makes it a pleasure to read. The first chapter takes us carefully through the various sources available to the archaeologist—texts, inscriptions, and “dirt” archaeology. We are then introduced to the earlier attempts at conquest by Julius Caesar—a near disaster for the great general—and given a sketch of the pre-Roman communities of the island. This is perhaps the weakest section of the book largely because the author has to contend with material currently undergoing a complete revolution in thought. He does his best, but explanations tend to look backward rather than forward. Then we are into the real meat of the subject; the opposing forces are sketched, and the course of the invasion across the lowlands of Britain is carefully traced. This is followed by a thorough summary of the actual archaeological evidence. Every belt buckle, apron-mount, and saucen handle of potential military significance is noted (and a number illustrated) to document the geography of the early military map. Finally, following the Claudian celebrations and the departure of Plautius, we are treated to seven short but helpful appendixes.

Graham Webster's book is to be strongly recommended: it is a work of scholarship, that gives both an insight into the workings of the Roman military machine of the first century A.D. and a splendid introduction to the archaeology of the Roman province of Britannia.

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STEPHEN JOHNSON. *Later Roman Britain*. (Britain before the Conquest: An Archaeological History of the British Isles, c. 1500 B.C.–A.D. 1066.) New York: Charles Scribner's Sons. 1980. Pp. xi, 195, 60 photographs, 22 maps, 20 figures. \$19.95.

This book is most attractively produced and is written in clear and lively English. Photographs are

excellent, maps and drawings graphic and informative. There is a useful list of sites and museums to visit. The notes and the one-page bibliography introduce the reader to specialist reports, often in journals likely to be unfamiliar to nonspecialists. The index, however, is totally inadequate: specimen omissions, even of proper names, include Abingdon, Butcombe (page 20 is included, but page 159 is not), Chysauster, Dumnonii, and so one might continue through the alphabet.

The work is in fact intended as a general introduction for the nonspecialist, forming part of a five-volume series entitled “Britain before the Conquest: An Archaeological History of the British Isles, c. 1500 B.C.–A.D. 1066.” The strength of the work is that it puts later Roman Britain into context, as the chapter headings demonstrate: “The Provinces of Britain,” “The Enemies of Roman Britain,” “Defence against the Barbarians,” “History of the Anglo-Saxon Invasions,” “The Arrival of the Anglo-Saxons,” and “Continuity of Settlement.” It is particularly useful to be reminded that there was no clear break between “Roman Britain” and what came later and to have an account of the Germanic invaders in their homeland and of the tribes beyond Hadrian's Wall.

On the other hand, as the title of the series implies, the emphasis is on the archaeological evidence, and literary and onomastic evidence, while not ignored, is perhaps not sufficiently integrated. The literary evidence for this period is notoriously obscure, but the archaeological evidence also frequently permits more than one interpretation. A crucial event is the migration or transportation of Cunedda and his people to North Wales. Stephen Johnson points out that there is no archaeological evidence for it but does not say what evidence he would expect. The date has long been controversial. For Collingwood, this was Stilicho's work, for Frere, Vortigern's, whereas Johnson sees the whole story as “a myth perpetrated by ninth-century Welsh scholarship” (p. 66), that is, by Nennius. His argument is not implausible but is hard to follow, unless the reader already knows what lies behind it: “By working back from the accepted date of Maelgwyn's accession in 534, the ninth-century historian has calculated 146 years from the magic date of 388, the death of the established Welsh hero who usurped a position as emperor of the Roman world, Magnus Maximus.” But 388 is 146 years before 534. How, one asks, could the historian have calculated otherwise? Johnson's skepticism here contrasts with his acceptance of the supposed British appeal to Honorius (pp. 105–07), for which there is no evidence at all.

In general, when one considers the type of reader for whom this series is designed, it might have been better to distinguish more clearly between what is

generally accepted and what is controversial. At times speculation gets out of hand. It is also to be noted that the emphasis, especially for the fifth century, is heavily on the South and Southeast. An account of Anglo-Saxon settlement and British resistance thereto should at least mention *Y Gododdin*. A reading of Nora Chadwick, *The British Heroic Age* (1976), not in the bibliography, would help to redress the geographical balance, as well as that between archaeological and other types of evidence.

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THOMAS S. BURNS. *The Ostrogoths: Kingship and Society*. (Historia, Einzelschriften, number 36.) Wiesbaden: Franz Steiner Verlag. 1980. Pp. 144.

The Ostrogoths were one of the major Germanic tribes, appearing in the light of Roman historical sources around A.D. 200. As a relatively coherent group, their history runs from this point down to 554, when the Ostrogoth kingdom in Italy was dispersed by Byzantine armies. Older generations of scholars tended to think of Goths, and other Germans, as well-formed tribal units complete with stable monarchies and fixed social systems. More recent work shows this not to have been the case at all. German tribal society changed continuously. And that, after all, is the real history of the early Middle Ages.

Thomas S. Burns's monograph follows this line. This is an exemplary work that might be read with profit by researchers not only in the period it covers but also in other fields. It is a case study of acculturation and social evolution backed with solid historical evidence. Anyone interested in the frontier experience will find this work of interest.

The author's theoretical base is stated at the outset: the word "tribe" is not so much a definition of a type of permanent social organization as one of a series of stages of social evolution. "[T]ribal evolution from more or less amorphous groupings to a chiefdom [kingship] proceeds as a result of outside stimuli" (p. 1). Germanic societies were tribal but under the impact of Rome; mainly, they evolved "until they were not tribal at all" (p. 1).

Burns traces the Goths from their days as small kin-bound groups lying north and east of Roman territories. By the later fourth century, under pressure from Rome and with Huns at their backs, Goths formed regional confederacies. Ostrogoths and Visigoths were similar peoples with "religion, politics, and tactical military operations the preserves of local and regional units. Great men . . . were overlords becoming kings" (p. 2). After a period of Hunnish domination, Ostrogothic society began to crystallize. By the next century it had developed a firm kingship drawn from a royal

family and clear social stratification based on family ties and wealth. In Theodoric's Italian kingdom, Goth and Roman fused at many levels. Gothic aristocrats were rapidly acculturated. Ordinary Gothic freemen seem to have become one with the lower orders. Within a few generations, and sooner, Goths had totally merged with Italians. Such social malleability was one of the hallmarks of tribal life.

Burns uses an impressive array of scholarly work to bolster his study. Readers will recognize debts to E. A. Thompson's and J. M. Wallace-Hadrill's work on early Germans. Better yet, the author draws on such fields as archaeology, historical geography, and anthropology (although one is a little surprised not to find Elman Service's *Primitive Social Organization* cited here) to more fully explicate Gothic history.

Additionally, readers will be treated to nuggets of careful scholarly judgment on various controversies and authors along with wonderful bits of social history. One example of the latter is the comparative inventory of upper- and lower-class Gothic households in Italy dated to 564.

This book is very well written, but nonspecialists will have to have maps of the Roman frontier and the modern Danubian and sub-Carpathian areas of Europe. The scholarly apparatus is very impressive, although the bibliography is slightly dated. In all events, this is a fine work, an example of the way that early history should be done.

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MEDIEVAL

GEORGES DUBY. *The Three Orders: Feudal Society Imagined*. Translated by ARTHUR GOLDHAMMER. Foreword by THOMAS N. BISSON. Chicago: University of Chicago Press. 1980. Pp. x, 382. \$25.00.

In the course of this extraordinary book, Georges Duby describes Jean Flori's studies on the medieval practice of dubbing as "the history of a meaning, a significance, and of the imperceptible changes that it underwent." The same can be said of *The Three Orders*. The "meaning" investigated concerns that paradigmatic image of medieval social structure expressed in the formula, familiar to every student of medieval history, that medieval society was divided into "those who pray, those who fight, and those who work." *Oratores, bellatores, et laboratores*: three orders, defined in terms of their functional contributions to medieval society, hence a trifunctional figure of thought.

Duby seeks to explain the history of this social construct in northern France from its initial formulation in the third decade of the eleventh century until its adoption as part of the apparatus of royal

ideology in the early thirteenth century. Appearing first between 1025 and 1031 in the writings of two northern bishops—Gérard of Cambrai and Adalbero of Laon—the trifunctional figure was already out of date, a conservative response to social change and the threat posed by competing models of social organization advanced in monasteries (especially Cluny), assemblies of the peace of God, and heretical movements. Against the underlying egalitarianism of its competitors, the trifunctional model drew upon the patristic thought of Augustine, Gregory the Great, and, above all, Dionysius the Areopagite to assert the necessity of social inequality and the hierarchical subservience of the lower order (the *laboratores*) to the needs and interests of the dominant classes (the *oratores* and *bellatores*). Particularly through their application of Dionysian theology, the northern bishops explicitly linked the three orders with exemplary structures of the heavenly Jerusalem and thus sacralized the barriers that separated one class from another. Social mobility, the confusion of clerical and knightly functions (looming in the Cluniac attempt to impose monastic values on the knightly classes), and the vain hope of heretics for a world without social distinctions were all neatly countered by a divinely ordained scheme intended to stiffen the will of a foundering monarchy or, failing that, to replace the king with the bishop as the pivotal figure of authority in medieval society. Thus, in contrast to LeGoff and Dumézil, whose work he amply acknowledges, Duby maintains that it was not the progress but the crisis of royalty in the late Carolingian period that led to the emergence of the trifunctional figure.

Curiously, the idea of trifunctionality, once articulated, almost immediately went into eclipse in northern France, not to reappear for another century and a half. When it did surface again, it had shed its episcopal garb and donned aristocratic trappings, addressing itself to the ideological requirements of the lay aristocracy in whose vernacular histories and books of counsel it was revived. Here trifunctionality promoted aristocratic independence from royal power and clerical morality by insisting upon the unique and crucial role of the warrior classes in medieval society and consequently upon their right to the goods and services of those who sustained them with prayers and labor. An implicit goal of the aristocratic revival was, it would appear, to justify seigneurial exploitation of peasants grown resistant to the demands for enhanced production placed upon them by their aristocratic lords.

The significant achievement of the aristocracy's use of trifunctionality was to desacralize it, thereby making it available as a legitimizing ideology for the performance of those secular tasks that, increasingly, a revived monarchy fought to monopolize. With the triumph of the French king, Philip Augustus,

over the feudal aristocracy at the Battle of Bouvines in 1214, the tripartite scheme became the banner under which the monarchy proclaimed its necessary domination over all classes of French society. Henceforth, the three orders—the three estates of the Ancien Régime—would persist as an institution of the monarchical state.

As this brief overview should make clear, Duby's *The Three Orders* is not simply a history of the idea of trifunctionality. More importantly, it is an attempt to trace the ways in which the meaning of the trifunctional model changed according to the social location of its user, whereby it came to validate entirely different sets of propositions about the ideal nature of society. *The Three Orders* is thus also a book about the interaction of mental and material realities, about how a "dream," an "imaginary scheme," was employed by threatened elites to legitimize their exercise of power. What accounts for the rise, diffusion, and altering significance of the trifunctional figure are deep-flowing structural changes in medieval economic, social, and political life that one after another placed different social groups in jeopardy and led them to seek a bulwark against change in a stable image of social hierarchy. For Duby, the trifunctional image is a "latent" mental structure, part of the collective consciousness of Indo-European peoples, pressed into service at particular times and places when circumstances favored its articulation.

It is the great merit of this book that Duby does not confine himself to the positive evidence of the trifunctional theme in the writings of northern French thinkers. (He has, in fact, provided an outline for its history in England.) The varieties of tripartite images that appear abundantly in all medieval thought—moral, political, juridical, theological—are reviewed with an eye to how they contributed to or impeded the adoption of the specific notion of trifunctionality, itself in turn skillfully grounded in the social and political contexts that called it forth.

It is doubtful if anyone but Duby could have written this book in such a richly compelling fashion, for it builds upon decades of research into the social and economic history of medieval France and brings to brilliant fruition his more recent investigations into the mental representations that governed and gave meaning to social life. The translation by Arthur Goldhammer captures much of the complexity of Duby's writing and, apart from a few infelicities due to a certain literalism, serves Duby well. Although scholars may quarrel with certain of Duby's readings of texts (this writer, for example, does not find a clear tripartition in Guillaume le Breton's first account of the triumphal feast after Bouvines), the book will clearly take its place among the masterpieces of French medieval history. It is a

tribute to Duby that *The Three Orders* makes us want to know more, to follow the history of trifunctionality into all its byways, both elsewhere in medieval Europe and at other times in France. As late as the nineteenth century, Baudelaire could still write: "There exist three beings worthy of respect: the priest, the soldier, and the poet. To know, to kill, to create." One would love to know what Duby might say about that.

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KARL BOSL. *Europa im Aufbruch: Herrschaft—Gesellschaft—Kultur vom 10. bis zum 14. Jahrhundert*. Munich: Verlag C. H. Beck. 1980. Pp. 419. DM 68.

More than forty years have passed since the publication of Marc Bloch's *Feudal Society*, and we need a new synthesis of medieval social history. Few scholars are better qualified to do this than Karl Bosl, the dean of German medievalists. This book is a comparative analysis of long-term structural changes in European society and culture between the late tenth and early fourteenth centuries, what Bosl calls the period of awakening. Bosl focuses on the Italian commune; the triumph of the Capetian monarchy over the feudal aristocracy; the persistence of "archaic" forms of lordship in Germany and their role in the rise of the ministerials, burghers, and peasants; and the shift in power from the king to the nobility among the Western Slavs. In spite of national and chronological differences in the tempo and degree of change, the emphasis is on the basic unity of European civilization, whose essential traits are freedom, individualism, and rationalism. This is a work of immense erudition, filled with penetrating observations.

Europa im Aufbruch reads at times like an updated version of Henri Pirenne's *Economic and Social History of Medieval Europe*. Bosl traces the origins of the European awakening to the cessation of the invasions; the expansion of trade in the North, Baltic, and Mediterranean seas; and the revival of urban life that gradually affected the countryside. This approach ignores the changes in agricultural methods and productivity that may have attracted the invaders to Europe in the ninth century, and it is perhaps significant that Lynn White is not listed in Bosl's extensive bibliography. Indeed, there are some peculiar omissions in that list. In spite of Bosl's interest in the French monarchy and laicization, only one of Joseph R. Strayer's articles is cited, and, although Bosl relies heavily on Georges Duby's classic study of the *Mâconnais*, he omits Duby's revision of his findings that appeared in the *Annales* in 1972. As a result, Bosl views the French knights

of the twelfth century as a new group in society, while Duby concluded that they were the descendants of people who had earlier been called nobles. Oddly enough, Bosl pays little attention to Karl Schmid's studies of the breakup of the large noble clans into separate lineages during the tenth and eleventh centuries. The domestic crisis that the Salians faced even before Gregory VII's intervention is thus never made clear.

I have always found Bosl's work stimulating but unreadable. Regrettably, this book is no exception. Bosl likes technical and non-German words, including such bastardized English words as *ausgepowert* (p. 255). The sentences and paragraphs are long, convoluted, and jammed with often unrelated facts. The book is repetitive and poorly organized. St. Francis is treated, for example, in the section on the German peasantry. A typical paragraph (pp. 23–24) discusses the lack of monastic sources in the tenth century, continuity between antiquity and the Middle Ages in northern Italy, and changes in Scandinavian society. Such stream-of-consciousness writing can leave the reader bewildered. The basic problem is that Bosl presupposes that the reader's knowledge is as extensive as his own. Bosl comments, for instance, that, unlike Planitz, he does not believe that the self-governing German city was the product of a revolutionary movement (p. 205). Unless the reader is familiar with Hans Planitz's analysis of the *coniunctio pro libertate* in Cologne in 1112, the reference makes little sense. The lack of footnotes compounds the problem. This is an important book, perhaps even a brilliant one, but anyone who understands it does not need to read it, and those who should will probably give up in frustration.

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MARY G. CHENEY. *Roger, Bishop of Worcester, 1164–1179*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1980. Pp. xvi, 397. \$59.00.

This is not really a biography of Roger, for which the surviving materials are inadequate despite his having been the son of Robert, Earl of Gloucester and hence first cousin to Henry II. Rather it is a pointed, and superbly pointed, study. The picture of the medieval English church that, owing largely to the work of students like Mary G. Cheney and C. R. Cheney, has now appeared makes plain that what we would expect of "An English Bishop in the Age of Becket" (a subtitle on the dust jacket that is curiously lacking from the title page) is a strong awareness of, and probably expertise in, legal matters. Because Roger was pre-eminent among English bishops of his time as a papal judge-delegate

and because the late twelfth-century canonical collections have been so thoroughly studied (some thirty are cited by Cheney), we can get quite a good idea of the legal side of this bishop's work. Of the six chapters that comprise the body of the book, two, occupying almost half the total space, are concerned entirely with legal matters. As for other aspects, there is virtually no information about spirituality or liturgy and very little about the cultural activity notable during Roger's episcopate. But important light emerges from the chapter devoted to the Becket affair in which the intricacies of Roger's part are now made plain, and the careful descriptions of individual cases that the bishop judged add a good deal to our understanding of the economic life of the church in this period. Throughout the book the level of technical scholarship is virtually without flaw.

The most outstanding features of the book, however, are the first two appendixes, which between them take up roughly a third of the work. The first presents the *acta* of Bishop Roger, with related documents. Eighty-three pieces are included, either in a full text (where originals survive) or calendared in English. With an economy reflected in the succinctness of her narrative, Cheney here lays out many of her sources, thus increasing the debt of gratitude students of the period will owe her. Appendix 2 is a calendar of letters of Pope Alexander III to Bishop Roger and of evidence for lost commissions to the bishop as judge-delegate. This provides an invaluable conspectus of the surviving part of the voluminous correspondence (126 documents are calendared here) between the pope most central to the development of the papal judicial system and the prelate he seems to have relied on most heavily—perhaps in the whole of Christendom—to judge appealed cases. Like the first, this appendix is a model of concise presentation of evidence. Leafing through it, the reader may easily get the impression that he could almost have done it himself: lay out the sources, and the book is as good as written. Such, however, is scarcely the case. Whether in the often very readable main narrative or in the cleanly presented bricks and mortar of the appendixes, the hand at work is a masterly one.

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SALLY N. VAUGHN. *The Abbey of Bec and the Anglo-Norman State, 1034–1136*. Woodbridge, England: Boydell Press; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. viii, 168. \$40.00.

Sally N. Vaughn has translated six documents relating to the first century of the Norman abbey of Bec, including the *Lives* of the first four abbots (Herluin,

Anselm, William, and Boso), the *Life* of a prior who became archbishop of Canterbury (Lanfranc), and a defense of the monastery's privileges against encroachments in the 1130s (*On the Liberty of the Abbey of Bec*). The *Lives* of Herluin, Anselm, and Lanfranc were written decades after the deaths of their respective subjects and reflect twelfth-century preoccupations as much as the eleventh-century milieu of their heroes. The *Lives* of William and Boso were composed by contemporaries and are brief, sober examples of their genre. Finally, the tract *On the Liberty of the Abbey of Bec* argued that the abbot of Bec "never did homage to anyone for the possessions of his church" and "never made a profession to any bishop" (p. 134). This theme of exemption from the archbishop of Rouen and the duke of Normandy ran through the other documents, and taken together they constitute a dossier for the defense of the "liberties of Bec" against threats from the growing power of the archbishop and the duke. The translations are clear and readable; the documents are unfortunately repetitious because several authors copied parts of their predecessors' works.

The translations are introduced by a sixty-page essay, divided into four chapters, on the rise and fall of Bec during its first century (1034–1136). The author's intention was to "concentrate on the political activities of Bec's abbots" (p. vii). This section of the book is less satisfactory than the translations for at least two reasons. First, the author wrote about Bec primarily on the basis of the six documents that she translated. Such a data base was too narrow to support a study with the ambitious title of *The Abbey of Bec and the Anglo-Norman State*. The large modern bibliography on Bec, monastic reform, the religious ferment of the eleventh century, Lanfranc, Anselm, and the development of the Norman duchy was largely ignored, aside from a few references to secondary works. One consequence of the artificially constricted data base was that the author had to rely on verbs in the subjunctive mood ("may well be," "seems," "might," pages 14–15, for instance). Second, the author attempted to find a guiding thread through the documents and through Bec's first century by reference to something dubbed "the reform tradition of Bec," which was comparable to but separable from the Gregorian reform (p. 47) and the Cluniac reform (p. 24). This "reform tradition" was said to constitute a "new order" built on the precedents set by Herluin and extended by Lanfranc, [which] was infused with reforming ideas based on the 'liberties' of Bec and the moderate Cluniac reforming ideas which Lanfranc encountered and adopted at Rome" (p. 41). In the author's view the reform movement emanating from the abbey of Bec was spread by the network of Bec monks who went on to abbaties and bishoprics.

Bec was certainly an important monastery, if only

because the Norman Conquest opened good careers to Lanfranc, Anselm, and a number of lesser figures. The author's argument about the existence of a distinct Bec reform tradition, however, is not supported by the documentation that she provided. The "liberties" of Bec, so vigorously defended in the documents, were not a plan for reform. They were privileges of the abbot that had arisen accidentally in the confused period (1034) when Herluin founded his house. By pure chance he had expressed subjection neither to the archbishop of Rouen (the see was vacant) nor to the duke of Normandy (he had just died). The Bec monks were defending these relatively narrow, parochial interests from the homogenizing forces of the twelfth-century church and state. They argued on the basis of history and custom; hence the self-serving recitation of the circumstances in which each long-lived abbot entered his office. This was no defense of a distinct reform tradition. It was one of those struggles over exemptions and privileges that were the spice of life to many a twelfth-century monk and canon.

Thus the introductory material in this book falls between two stools. It is too elaborate to serve merely to introduce the translated texts but too brief to fulfill its ambitious goal of treating the political role of Bec in its first century. The translations themselves are potentially useful to students and scholars, and the author is to be commended for the labor expended on them.

JOSEPH H. LYNCH
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J. ROBERT WRIGHT. *The Church and the English Crown, 1305–1334: A Study Based on the Register of Archbishop Walter Reynolds*. (Studies and Texts, number 48.) Toronto: Pontifical Institute of Mediaeval Studies. 1980. Pp. xix, 472. \$27.00.

This richly documented work is distinguished by mature scholarly judgment and grounded in long-sustained and painstaking research. In it J. Robert Wright examines the tripartite relationship between the crown, the English church, and the papacy during "the formative period of the Avignon papacy" at a time "when the relations of England with France were not yet clouded by the full impact of the Hundred Years War" (p. x). The book is divided into three parts concerned, respectively, with papal provisions, the defense of the royal prerogative in the context of papal claims upon the resources of the English church, and the internal governance of that church in areas affording opportunities either for conflict or cooperation with the crown. Thirteen appendixes, taking up no less than 140 pages, follow the main body of the text. These appendixes, which appear to have been very carefully done, contain a

mine of information, and scholars have particular reason to be grateful for the tables of papal provisions to English and Welsh benefices during the Avignonese era and for the meticulously referenced itinerary (some fifty pages long) of Walter Reynolds, Winchelsey's successor (1318–27) as archbishop of Canterbury.

Although the book is based on a broad array of sources, unprinted as well as printed and documentary rather than chronicle, Wright has relied mainly on Reynold's episcopal register, perhaps the principal source for fourteenth-century English ecclesiastical history to remain unpublished. It is fitting, then, that one of his conclusions should be the claim that Reynolds deserves from historians a somewhat less unfavorable assessment than he has hitherto received. The allegations of hostile chroniclers notwithstanding, he appears to have been neither simoniac nor unlettered, and he emerges from these pages as a competent administrator and a reasonably effective moderate in his dealings, after the turbulent primacy of Winchelsey, with both king and hierarchy. "Indecision is perhaps the strongest charge we can lay to Reynolds, but indecision may not be a vice in a primate whose king is foolish and whose pope is clever" (p. 276).

Wright's further reassessments are comparably judicious in tone. Thus, in the matter of papal provisions, John XXII's constitution *Ex debito* lacked the importance Deeley once claimed for it and (*pace* both Deeley and W. E. L. Smith) *Execrabilis* "in the long run did not create any permanent or significant new source of papal provisions" (p. 93). It was the English episcopal hierarchy that gained most by it, just as they gained also by securing papal faculties to make provisions themselves—a tactic that led, Wright claims, to the emergence of "a secondary, rather extensive network of provisions only one step removed from the pope himself" (p. 67).

Of course, as Pantin pointed out some years ago, the English king came to be accorded similar faculties later on in the century, and it is the principal contribution of Wright's welcome focus on the earlier and less well-studied years that it confirms and strengthens the impression (conveyed already by Lunt, Highfield, Pantin, and McKisack) of the notable success enjoyed by the English kings of the day in their dealing with both church and papacy. That contribution is an important one, and the book deserves a strong welcome from all scholars interested in the late medieval church.

FRANCIS OAKLEY
Williams College

PIERRE CHARBONNIER. *Une autre France: La seigneurie rurale en Basse Auvergne du XIV^e au XVI^e siècle*. In two volumes. (Publications de l'Institut d'Études du Mas-

sif Central, number 20.) Clermont-Ferrand: Institut d'Études du Massif Central. 1980. Pp. v, 668; 669–1,293.

Pierre Charbonnier's study of rural society in the western mountains of Basse Auvergne is a major contribution to the historical literature on France in the late Middle Ages and sixteenth century. For Charbonnier, historians have paid too much attention to the rich provinces of France. The story of the decline of noble families, the crisis of feudalism, the collapse of seigneurial revenues, and the penetration of urban wealth into rural areas is true enough for many of the richer areas of France. It is now time to look at the poorer areas, "the other France" where the seigneurs remained fully in control. Charbonnier also feels that many historians have gone overboard with quantification and have made "grandiose deductions from minor variations in numerical series" while in the grip of quantifier's "vertigo" (p. iii).

Charbonnier's picture of Basse Auvergne is refreshingly different. On the eve of the Black Death, population density in the western mountains was about average for France at the time, but the polarization of peasant society into rich and poor found in more economically advanced areas was conspicuously absent. Peasant society was uniformly poor. The majority of households had about enough land to provide for a family in normal times. Neither *coqs de villages* nor landless day laborers were numerically important. The center of peasant life was the *mas* or *manse* with its commons and scrub pasture. Parishes were somewhat artificial units and the real binding force holding the dispersed settlements together was the seigneurie.

The seigneurs were for the most part financially sound and drew their principal revenues from peasant tenants. Since the great majority of dues were still paid in kind, the seigneurs escaped the crisis of seigneurial revenues so common in more economically advanced areas where commutation to cash payments exposed revenues to the erosion of inflation. Seigneurial management was in the hands of stewards (*receveurs*) who shared room and board with other live-in servants under the supervision of the seigneur. Leasing of seigneuries was virtually unknown, and some seigneurial dues were still levied by administrative vassals (*vagiery*). In short, early fourteenth-century Basse Auvergne was reminiscent of conditions in more advanced areas two centuries earlier.

The Black Death swept through Basse Auvergne and soon the armies and bands of marauding soldiers appeared, but by 1390 the worst was over. During the troubles, the seigneurs acted as the protectors of their peasants and thus reinforced

seigneurial ties. The seigneurs also directed the reconstruction of the countryside, recruited locally for resettlement, and at most granted temporary reductions of seigneurial dues. In contrast to many areas of France where the seigneurial system grew weaker in the fourteenth and fifteenth centuries, in Auvergne it emerged virtually unchanged. In the absence of strong market pressures, local needs for traditional foodstuffs led to a return to traditional farming and tenancy arrangements.

There was no crisis in the seigneurial system here in the late Middle Ages. At most, the lowest reaches of the nobility, families without seigneuries, faded away, while the seigneurial families tended to hold on and even to consolidate their properties. Domains or seigneurial reserve lands were still relatively unimportant except for a few dairy farms that belonged to the nobles in the high pasture mountains. The only sign of urban interest in rural land in the fifteenth century involved these same pasture mountains, especially the eastern face of the western mountains that attracted residents of Clermont and Montferrand.

The fifteenth century was a golden age for peasant society in western Basse Auvergne apart from the visitations of the plague common to all of France. Communal families working large farms appeared throughout the area. Most communal arrangements involved the heirs of a single tenant invested with a large farmstead at the beginning of the fifteenth century. The heirs lived together to hold the farm intact. Additional communal families formed in the early sixteenth century, but by then most were disintegrating from the tensions of multi-family life.

The most important changes in rural life came in the sixteenth century, but even here they emerged in slow motion and attenuated form. Among the peasants, population pressure led to land fragmentation, but for the first time significant differences in wealth occurred in the heart of the rural peasant communities. Rising agricultural prices and lagging wages provided the economic incentives for modest concentrations of land. Peasants were among the most active buyers of land, and by the end of the sixteenth century western Basse Auvergne had its version on a modest scale of the *coqs de villages*. Seigneurs also participated in land concentration both for grain production and for dairy farming, while urban dwellers increased their share of the high pasture mountains. All this was on a small scale, and the peasants of western Auvergne held on to a much greater proportion of land in the sixteenth century than their counterparts in Ile-de-France or Picardy. Significantly, the only emigration of any importance involved strictly seasonal agricultural work.

Sharecropping became the most common form of nonseigneurial land tenure in the sixteenth century, but the strictly local markets for grains provided few opportunities for capital accumulation for farmers. Sharecroppers remained poor and were in no way comparable to the capitalist tenant farmers of northern France. Indeed, the chances for the accumulation of wealth and for upward social mobility were very limited for those working the land. Social advancement usually required the abandonment of farming.

One of the most fascinating chapters of Charbonnier's study traces the emergence of what the author calls the rural notables, a term he understandably prefers to the more common, but confusing, title of rural bourgeoisie. The ambitious and lucky left the peasantry to become notaries, officials in the seigneurial law courts, seigneurial managers, and moneylenders. Little by little these notables acquired land that they then leased profitably to sharecroppers.

The seigneurs also grew more prosperous in the sixteenth century as they added commercial farms (*domains*) to their traditional seigneurial revenues. The latter were still predominantly in kind and hence the seigneurs lost nothing to the sixteenth-century inflation. Seigneurial justice still handled the majority of legal cases and the rural seigneurie as a whole showed no serious signs of decay. Resident seigneurs still relied on *receveurs* and the practice of leasing entire seigneuries was common only for ecclesiastical properties.

In short, Charbonnier presents a major rehabilitation of the rural seigneurs. He also treats in abundant detail many other absorbing topics—rural priests, agricultural production and cropping systems, subsistence crises, the plague, crime, diet, and so on. The author writes without ideological bias and makes every effort to integrate his findings into the vast literature on rural France. Introductory chapters even contain in outline form the history of rural western Basse Auvergne in the seventeenth, eighteenth, and early nineteenth centuries.

The only drawback of Charbonnier's splendid study is its immense length (1,293 pages) that will probably discourage all but specialists. Perhaps an abridged edition of more manageable proportions will appear and help generalize his findings for one of the regions of "the other France."

JAMES L. GOLDSMITH
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MANFRED LAUFS. *Politik und Recht bei Innozenz III.: Kaiserprivilegien, Thronstreitregister und Egerer Goldbulle in der Reichs- und Rekuperationspolitik Papst Innozenz' III.* (Kölner Historische Abhandlungen,

number 26.) Cologne: Böhlau Verlag. 1980. Pp. viii, 335. DM 88.

The Carolingian and Ottonian privileges by which Innocent III held the papal estates at the time of his election in 1198 were quite insufficient as legal grounds of proprietary right. The protection of those papal claims, furthermore, was no longer the fundamental purpose of imperial policy in Italy. The steps that led to the alliance between Innocent and the Guelph candidate for the imperial throne, Otto IV, were collected *ex parte* in the *Registrum super Negotio Imperii* or RNI, now *Registra Vaticana* 6, a phalanx of documentation designed originally to compel Otto's fulfillment of his treaty promises, which eventually became the basis for the pope's charge of natural law illegitimacy against him, the justification for his deposition by the German princes, and the major source for the imperial privilege granted by the temporarily complaisant successor, Frederick II, in the Golden Bull of Eger.

Building on the paleographical, textual, and legal foundations laid especially by Friedrich Kempf in three volumes of *Miscellanea Historiae Pontificiae*, Manfred Laufs has produced a work of documentary political history of the strictest kind, with little or no regard to the events and necessities of the wider world outside the writing offices of the pope and emperor and no attempt at a comparison between the imperial *Thronstreit* and Innocent's political campaigns against John Lackland and Philip Augustus. Perhaps the book should bear its subtitle instead of its title, which promises too much. The narrow focus itself is not a fault, however; it permits an elegant and cogent demonstration of the means and stages by which Innocent III forced an epoch, elevating his possession of the papal estates from a pseudopatrimony by imperial warrant to a real sovereignty, simultaneously with and by means of the development of a political secretariat and registry alongside the old chancery.

Part of the fluent argument is a very useful examination of the RNI itself, a unique moment in the development of papal administration and of decretal law and archival practice, the beginning of standardization and consistency of policy from pontificate to pontificate that made the Apostolic See so formidable among the sovereignties of Europe for the following two centuries. Another enlightening contribution is Laufs's essay on the Christian and Roman traditions of natural law as enforced by Innocent against Otto. The pope, in charging the emperor with ingratitude and faithlessness, was not just solemnly venting his anger, but setting out the natural law complaint that ended in an effective sentence of deposition.

The book is up to Böhlau's normal high standards

of production, but the index is disappointing, sketchy, and misalphabetized.

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MODERN EUROPE

RICHARD LANGHORNE. *The Collapse of the Concert of Europe: International Politics, 1890–1914*. New York: St. Martin's Press. 1981. Pp. xv, 137. \$18.95.

Anyone who likes a book in which the author is talking to himself, wondering aloud about the book he is going to write someday, will love this one. For that is what this is in essence, a writer's shorthand notes to himself. And they are carelessly written notes, so full of awkward or incomprehensible passages that he will have trouble sorting things out later. One favorite: a sentence opening a chapter that reads, "It has been some time since historians began to attribute less importance to non-European affairs in the history of nineteenth-century international politics." But there are other observations, equally original, such as the corpulent Edward VII "encircling" Germany all by himself, or "There can be no doubt that Francis Joseph would not tolerate real national gains, but it is clearer to us than to his advisors that he could not either." Where was the copy editor at St. Martin's, out to lunch?

Not that the book is without virtues. The author, Richard Langhorne, who is a Fellow of St. John's College at Cambridge, can be intelligent as well as obscure. Thus he offers a good definition of the concert as the assumption that the major nations of Europe were roughly equal in power, and that basic changes were either undesirable or to be brought about by negotiated agreement only. Or there is his suggestion that it was not just the emergence of Germany that altered the concert, but that larger forces did so as well, such as the new technology, which vastly enlarged the concert's defined area, or the relative decline of Europe that resulted from the emergence of Russia and the United States. But the bright spots are hidden; they are throwaway lines. The bulk of the book consists, in its first part, of author's comments—with hardly any explanation or narrative—on the diplomacy of the period, and in its second part, of a more traditional diplomatic history. But in either part, one is puzzled by just who the intended audience might be, whether colleagues or students. The scholar will have read these things many, many times before, and it is difficult to imagine a student whose head will not swim as the author skips all over the diplomatic landscape, from the Crimean War to the Straits Convention of 1841

to the occupation of Tunis forty years later to Bismarck's grand design and back to Napoleon's Egyptian campaign, all that and more in the space of one four-page section. Nor does he offer even the most basic sort of guidance. You are supposed to know what the Treaty of Shimonoseki was, and the diplomatic protagonists are hardly ever introduced beyond a mention of their bare last names. The ambassadors, foreign ministers, and minister-presidents of the period are supposed to be household names. So are those of historians, by the way. A. J. P. Taylor, when mentioned in the text, loses his initials—"Taylor has written"—after all, it is such an uncommon name.

In a way, all this is a pity, for the occasional insight makes one feel that the author has something to say. But he is not saying it here.

JOACHIM REMAK
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Santa Barbara

DIANE WILLEN. *John Russell, First Earl of Bedford: One of the King's Men*. (Royal Historical Society Studies in History, number 23.) London: The Society. 1981. Pp. xi, 145. \$31.85.

Diane Willen's work, the latest volume of the "Royal Historical Society Studies in History" series, is a readable and carefully documented study of an important if not dazzling contemporary of Henry VIII. Willen's work is limited in scope, for she makes no attempt to deal with the Tudor period as a whole. Rather, she is interested in assessing the career of John Russell, some aspects of which have been largely ignored by prior historians. In this limited way her book makes a valuable contribution to the historical literature of the period.

John Russell, first Earl of Bedford (1485–1555), one of Henry VIII's new nobles, was in every sense a true *politique*, a shrewd and practical man able to survive and profit in dangerous times. Russell's political career rested on his loyalty and diligence. Early in his career he became a friend of Wolsey and of Henry VIII and indeed spoke out for Wolsey, thus incurring the enmity of Anne Boleyn. Despite her displeasure, however, his friendship with Henry lasted, and Russell was frequently employed in diplomatic, administrative, and military capacities. Hardworking and honest, he enjoyed a reputation for integrity among his contemporaries, and these attributes led eventually to appointments as privy counselor after 1536 and as one of Henry's new nobles after 1539.

In assessing Russell's career, Willen notes that, although in many ways the earl was not an outstanding figure, he nonetheless achieved great personal success. His name cannot be associated with any

single line of state policy, piece of legislation, or institutional legacy. Although in later years he showed ingenuity in governing the western counties, Russell basically was a man who preferred to serve unobtrusively, to carry out policies rather than to initiate them. As a member of Henry's council, however, and as lord lieutenant of the West, Russell used profit, friendship, and patronage to amass a large personal fortune. Having been granted the lands of Tavistock upon the dissolution of the monasteries, Russell so shrewdly managed both landed and mercantile interests that his son could be counted among the twenty-three wealthiest men of the realm.

Personal aggrandizement of wealth and steadfast service to the crown seemed to satisfy Russell's ambition. He never, writes Willen, made any attempt to create a party or a personal following at court. Furthermore, despite his obvious self-promotion, he was neither wholly mercenary nor opportunistic. His loyalty to the crown—not just to Henry VIII but to Edward and Mary as well—stemmed in Willen's estimation from the deep-seated conviction that a strong monarchy and undisputed succession were the only means of political stability in precarious sixteenth-century England. On the religious issue Willen finds that Russell was not motivated by religious ideology at all but by an Erastian fear of civil war.

In discussing her research on Russell, Willen notes that he has by and large been passed over by modern historians. Nineteenth-century studies are generally unreliable, and the respected works of Gladys Scott Thomson on the Bedford family did not attempt to discuss Russell's political significance and have ignored his achievements in management of the western counties. Willen bases her careful assessment on scattered sources, especially on state papers, the Cottonian manuscripts, the Exchequer records, Devon and Bedford office records, and work by the History of Parliament Trust. The result is a scholarly, thoughtful work about an honest, practical man, whose career "evolved slowly, included both failure and success, and must be judged as a whole" (p. 63).

WILLIAM W. MACDONALD
Lamar University

DAVID C. PRICE. *Patrons and Musicians of the English Renaissance*. (Cambridge Studies in Music.) New York: Cambridge University Press. 1981. Pp. xix. 250. \$55.00.

This book was conceived in the long shadow cast by Walter L. Woodfill's *Musicians in English Society from Elizabeth to Charles I*, but it is more than a footnote to that work. It has a different perspective and poses

somewhat different questions. It is more concerned with private patronage than with official. It undertakes to trace the development of a "genuine musical literacy" in Elizabethan and Jacobean England. In light of that development, David C. Price questions the skeptical attitude of recent scholars toward the idea, once widely held, that Elizabethan society was peculiarly permeated by music. He does not go so far as to suggest that wherever two or three were gathered together they sang five-part polyphony (another's *mot*, not mine). Rather, he attempts to trace a complex network of private patronage among county families, who contributed informally but effectively to musical education and appreciation by means not only of support given to musicians but also through the copying and exchanging of music in manuscript form. This informal traffic in manuscripts appears to have been an even more significant source of musical literacy than the book trade itself. What kept the movement at this informal, manuscript level, Price points out, was in part the Renaissance gentleman's fear of somehow losing his credentials of gentility by seeming too professional, either as performer or publisher.

In a society still experiencing the shock waves of the Reformation, music was bound to reflect the changing status of the church as a patron and the religious preoccupations of private patrons. Not that religious music overshadowed secular: the two in fact enjoyed a mutually supportive relationship. Nor did the Elizabethan church serve as a dominant source of patronage. The long debate over the place of music in the service led to compromises not likely to foster creativity. Yet the very ambiguity of the settlement made possible the retention of much that was associated with the old order. The Reformation tended rather to shift patronage from church to country seat, and musical composition from public worship to private devotion. At this level, Price claims to find little difference between recusants and Protestants. Protestant families helped develop the rich tradition of psalm singing; Catholics continued, more or less clandestinely, to perform music more nearly associated with the Roman rite. In both cases private patronage of religious music tended to stimulate interest in secular forms as well, insofar as the two can in fact be at all clearly distinguished. The Roman Catholic allegiance may have been especially important in fostering also the vogue for Italian musical literature of all sorts, but Price contends that its primacy as a musical influence remains unproven.

These and other insights come winnowed largely from a closer examination than has yet been made of family papers, letters, and accounts, which Price considers the best evidence available for an assessment of musical literacy. Despite the apparent hardness of this sort of evidence, it will not support more

than tentative conclusions, nor does Price pretend that it will. He wisely eschews quantification. Like other scholars who work with this kind of material and in this period, and who have of necessity had to deal selectively with whatever has managed to survive, Price finds himself torn between a positivism he would like to achieve, but cannot, and an impressionism he distrusts, but must resort to anyway. He has been able, however, within a limited scope, to contribute a useful element of substance to what has been, and no doubt always will be, an essentially impressionistic picture of Elizabethan and early Stuart society and culture.

ARTHUR B. FERGUSON
Duke University

J. V. BECKETT. *Coal and Tobacco: The Lowthers and the Economic Development of West Cumberland, 1660–1760*. New York: Cambridge University Press. 1981. Pp. xiii, 278. \$47.50.

Historians are aware that Cumbrian affairs in the seventeenth and eighteenth centuries were dominated by the Lowthers. Because until recently their records were relatively inaccessible, however, the careers of the family's most redoubtable members have not previously been closely examined. In remedying this omission J. V. Beckett reveals Sir John Lowther (1642–1706) and his son, Sir James (1673–1755), to have been not only the region's wealthiest and most influential proprietors but also the very models of land-owning entrepreneurs who were largely responsible for realizing West Cumberland's economic potential. His book contains much more, however, than a valuable study of one very large estate. Beckett has used the Lowther archive and much other material to provide a wide-ranging analysis of West Cumberland's economic history during this period, encompassing agriculture, industry, trade, communications, and urban development.

Coal was the staple from which most other developments grew. The availability of sea transport provided the opportunity to exploit a substantial overseas market for coal in Dublin. Sir James Lowther in particular accorded colliery development first priority: by 1750 three-quarters of his local revenue was derived from coal, and West Cumberland provided a similar proportion of the coal sold in Ireland. Besides other established trading links with the Baltic, the area was also well placed to benefit from a colonial connection, especially in the war-torn years after 1689 when ships from many other ports were more vulnerable to privateering. In the 1740s West Cumberland's main port, Whitehaven, was second only to London in the size of its tobacco import. Both these developments

prompted other activities (salt panning, glass making, and ore smelting, for example) and thereby stimulated the growth of both the local commercial community and employment generally. Harbor facilities and roads were improved, and, under the watchful eyes of the Lowthers, Whitehaven became one of the first planned towns in England since the Middle Ages. Development was not sustained beyond the later eighteenth century. The region failed to establish a specific role over and above coal mining; nor did population grow fast enough to produce the sizable consumer goods industries with which the national market could be invaded. Growth during this period, however, was substantial and varied, giving the lie to the romantic Wordsworthian notion (adopted by Macaulay and later accepted by Trevelyan) that this part of northwest England contained a static and poverty-stricken society until the nineteenth century.

Coal and Tobacco is well produced and contains numerous (though unobtrusive) maps, plans, and tables, as well as several highly informative appendices. While evidently based on painstaking scholarship, the text has been written with a light touch and contains comparative references to developments in Shropshire, Tyneside, Derbyshire, and elsewhere. It should be regarded as essential reading by anyone interested in the relationship between land ownership and economic growth or in regional economic development in the early modern period, and it is particularly commendable for the evidence that it provides of local prosperity in a period when many other areas, more dominated by agriculture, were experiencing economic depression.

PETER ROEBUCK
New University of Ulster

STEPHEN F. GRADISH. *The Manning of the British Navy during the Seven Years' War*. (Royal Historical Society Studies in History, number 21.) London: The Society. 1980. Pp. ix, 235. \$37.36.

Writing administrative history presents some serious challenges to the historian. The opportunities to be tedious are legion, and the likelihood of informing with style and grace is minimal. Happily, Stephen F. Gradish's *The Manning of the British Navy during the Seven Years' War* avoids tedium; it also constitutes a new form of administrative history by neatly dovetailing the interaction of politicians, bureaucrats, and the mass of humanity most directly affected by the policy- and decision-making processes of government.

Gradish's main concern is to explain how the ministers of George II and George III found the 80,000 or so mariners necessary to man British fleets during the 1756–63 conflict and how these

sailors were assigned to the various squadrons charged with taking the war to the French. As a result of his meticulous research, which makes measured and convincing use of the relevant printed and manuscript sources, we learn that the earl of Chatham deserves his reputation as a great war leader for directing Britain's resources against French weaknesses, but not for helping find the manpower so necessary to English offensives, and that considering the technical obstacles they faced, the administrators of the Admiralty deserve an acknowledgement that they have rarely received for contributing to their country's success in the war. In the course of making these two main points, Gradish provides insights into the machinery of Hanoverian government, the activities of press gangs, the perils of life at sea, and the state of naval medicine in the middle decades of the eighteenth century. These perceptions make his work admirable not simply as administrative history, but as political, social, and, to a certain extent, intellectual history as well.

The strengths of the book are many. In dealing with the issue of impressment, for example, Gradish explains why the press gangs were necessary, what legal authority they had, how efforts to find other means of manning the navy failed, and how critical manning was to the success of all military operations. Men were pressed not so much because the Admiralty was tradition bound or Englishmen were unpatriotic or cowardly. They were pressed rather because Parliament could not come up with an alternative means of manning the navy by conscription or voluntarism and because sailors preferred the mercantile service to that of the navy, where pay was significantly lower, dangers were omnipresent, and terms of service lasted for years rather than for a single cruise. In addition to his concise treatment of the impressment issue, Gradish deals with shipboard life—particularly its dangers—with a sensitivity and breadth of knowledge that is especially striking. Diseases such as scurvy and typhus, which in reality posed a far greater danger to the common seaman than French cannon, are dealt with in the context of a groping eighteenth-century medicine that treated by educated guesswork based on observable cause and effect. Dr. James Lind, the naval physician who was therapeutically far ahead of most of his colleagues, is not portrayed as a sage despised and ignored by the Admiralty but rather as a clever man whose suspicions about disease and contagion were largely correct but also were beyond the era's capability of scientific demonstration.

Just as the work has its strengths, there is also some room for improvement. There is too little correlation between administrative decisions and the military operations that they affected. It is, for example, made clear that Admiral Byng's loss of

Minorca was caused by insufficient manning and subsequent crew depletions, but exactly how the process developed is left to the reader to surmise. Moreover, aside from a few references to the French attempt to establish a national mariner's registry for military purposes there is little mention of how Britain's enemies coped with and were affected by the problem of manning their own fleets. A comparison, no matter how brief, of the manning techniques used by all of the naval participants in the Seven Years' War and their relative effectiveness might have cast the problems faced by the Admiralty in greater relief.

This is, all in all, a first-class book that should be read by all who hope to deepen their understanding of Hanoverian Britain. The scholarship is of the highest quality. It is written in a brisk style that rests in that enviable middle ground between ponderous and flashy, and its organizational balance highlights its many other virtues.

RONALD POLLITT
University of Cincinnati

PAMELA HORN. *The Rural World, 1780–1850: Social Change in the English Countryside*. New York: St. Martin's Press. 1980. Pp. 331. \$25.00.

Pamela Horn is the author of previous works on country children, rural education, and laboring life in the countryside and of a biography of the first leader of the agricultural laborers' union. She therefore brings excellent credentials to a book on the rural world. She is very familiar with day-to-day concerns of the farmers, agricultural laborers, and small tradesmen who made up the rural community beyond the great estates. Her book contains much useful information on village institutions, farming habits and customs, and country leisure and sport. She leads us sensibly and reliably through such controversial topics as enclosures, poor relief, and the rationalization of the rural economy. Assiduous research and well-chosen forays into county record offices and other collections of original sources lend substance to her even-handed approach.

A presentation of so many themes and issues has its disadvantages. The reader is occasionally jostled from topic to topic. Densely packed accounts can result in curious juxtapositions. Chapter 7, for example, lumps together Tolpuddle trade union martyrs, Chartist political agitators, and conservatively inclined agricultural protectionists. A more serious problem only gradually emerges as the chapters slip by. It becomes clear that the book's descriptive merits betray its stated aim—to explain social change in the countryside. Horn's descriptions are essentially static: social change is dynamic. Extracts from farmers' diaries, advertisements from provin-

cial newspapers, and snippets from the universally quoted William Marshall do not in themselves explain social change. Horn lacks an analytic framework that would organize her evidence into an effective explanatory instrument. The contrast with James Obelkevich's book is instructive: his sociological and anthropological approach not only brings new ways of thinking to bear upon the rural community, but it also presents a model consistent with an attempt to explain change (see the chapter on "Agrarian Society" in his *Religion and Rural Society* [1976]).

Without a framework, Horn's book is diffuse. Nevertheless, scattered throughout are the makings of a coherent picture of social change in the countryside. Time and again Horn turns her attention to the agricultural laborers. She describes their desperate economic plight. She refers to those peculiarly rural manifestations of social crime, poaching and incendiarism, in which laborers played a central role. Virtually all the illustrations and tables are concerned with laborers' social conditions. Horn obviously sees agricultural laborers as crucial to any discussion of social change in the countryside; it was among them, she writes, "that the greatest changes were apparent" (p. 255). Yet the consequences of rural poverty, increasing class stratification, and the imposition of various forms of social control are only hinted at.

Horn is a careful and scrupulous scholar. Had she made her latent theme manifest, her book could have been properly claimed as a work on social change.

TRAVIS L. CROSBY
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Norton, Massachusetts

EDWARD GILLET and KENNETH A. MACMAHON. *A History of Hull*. (University of Hull Publications.) New York: Oxford University Press. 1980. Pp. x, 428. \$45.00.

The first half of Edward Gillett and Kenneth A. MacMahon's *A History of Hull*, which covers the period from the town's earliest beginning in the eleventh century to the eighteenth century, is a fascinating tale of war, whaling, piracy, plague, press gangs, riots, and fortunes made and lost, as well as an account of the economic and political development of the town. Hull's closest ties were determined by geography and economics. The settlement began as a transshipment point where river boats from Beverley and York met the larger vessels from the North Sea and the Baltic. So closely tied was the town to the sea that it did not feel the need for roads until the early fourteenth century, and until the twentieth century ships dominated its life.

The port handled a wide variety of commodities, but until the eighteenth century its trade was mainly in wool, wheat, and wine. York was its strongest domestic influence and capital supplier, while the ports of the North Sea, the Baltic, and the Channel provided its main overseas contacts. As industrialism developed, the balance shifted and the West Riding took over as the chief domestic supply and market area, while international horizons widened. Fishing did not become important until the nineteenth century, when the city also added many processing and manufacturing industries to its traditional activities. In the twentieth century the effects of Hull's pivotal position between northern England and northern Europe were overwhelmed by the impact of new forms of transport, which acted on this city much as they did on any other, producing suburban sprawl and the depopulation and degeneration of the old city and commercial center.

Hull could be expected to provide an unusual case study of a settlement less insular and more open to foreign influence than most provincial English towns. From the earliest states of its existence its merchants and sailors ventured all over northern and western Europe, while it was visited by merchants from Italy and Russia and its connections with France became so important that by the fourteenth century several local families had developed branches in Bordeaux. In the nineteenth century the port acted as a funnel for a large portion of the migration from northern Europe to North America; in 1888 alone 41,000 migrants passed through. There are many tantalizing references to this cosmopolitan side of the city, such as the seventeenth-century traveler's note that the people of Hull could be distinguished from those inland by their "Dutch faces," and the comments on the Irish and Italian communities in the nineteenth century, on the would-be migrants who settled in Hull instead of crossing the Atlantic, and on the German, Russian, and Jewish communities. Unfortunately, there is no attempt to analyze the impact of these non-English influences on the life and development of the city. While disappointing in some respects and surprisingly interesting in others, this essentially solid and conventional biography of a city provides an excellent overview; it should help shape other studies of the unique aspects of northern English port life that it suggests but does not explore.

JANET ROEBUCK
University of New Mexico

F. K. PROCHASKA. *Women and Philanthropy in Nineteenth-Century England*. New York: Clarendon Press of Oxford University Press. 1980. Pp. ix, 301. Cloth \$48.00, paper \$21.00.

Here is an excellent book that should be read by all students of Victorian society and literature. "If we are to isolate one profession," F. K. Prochaska writes, "that did more than any other to enlarge the horizons of women in the nineteenth century it would have to be the profession of philanthropy" (p. 222). The point is backed by an impressive amount of research, yet it is one of the book's few large generalizations about "the Cause." Prochaska makes it clear at the outset that the origin of his research was an interest in philanthropy, not feminism, and though some women's historians may draw breath at a statement like "freed from many of their domestic duties women commonly found that there were few alternatives to idleness" (p. 5), his analysis does not in fact depend on the somewhat discredited "idle women" thesis. Nor does he consider class guilt or political fear to be chief motives among women, although sexual guilt is offered as a possibility—atonement for Eve's initiating the fall of man. Whatever the validity of this (and it seems a bit dubious to me), Prochaska is surely right to emphasize the importance of religion, and particularly the theology of love that he claims was especially prominent in women's understanding of the New Testament, as an explanation for the undoubted force of women's attraction to charitable work.

What is particularly valuable in this book comes in the detailed analysis of just what charitable work actually meant for women. Their early initiation in it is evidenced by the remarkable degree to which Victorian charitable entrepreneurship mobilized children, especially girls. Prochaska estimates that during the nineteenth century children must have raised well over one million pounds for the Methodist Missionary Society *alone*. Equally fascinating is the chapter on bazaars, which formed a sort of privileged area where women could take initiatives prohibited in normal circumstances. The bazaar episode in *The Mill on the Floss*, to name but one of many novels, is illuminated by this chapter. Another privileged area was the rescue and rehabilitation of prostitutes, woman's particular mission to women. Its more sedate aspect was the funding and management of Magdalene homes, which women took over increasingly from men through the century. Its activist end was the actual midnight mission work in the mean streets and brothels, where excitement (though not perhaps physical danger) as great as that faced by any heroine of melodrama could be experienced by adventurous women rescuers. The ambience seems not so far from that later generated by the suffragette movement, and Prochaska plausibly suggests that the sexual *esprit de corps* created by rescue work had implications for the women's suffrage campaign.

There are six impressive appendixes tabulating the financial contribution of women through sub-

scriptions and legacies to 141 charities for which subscription lists were available. These offer valuable evidence of women's charitable preferences (Dickens's Mrs. Jellyby is not entirely a freak; women *did* favor foreign missionaries) and financial power in general. Finally, there is an excellent forty-page bibliography.

CHRISTOPHER KENT
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JOAN N. BURSTYN. *Victorian Education and the Ideal of Womanhood*. Totowa, N.J.: Barnes and Noble or Croom Helm, London. 1980. Pp. 185. \$24.00.

Joan N. Burstyn examines the movement for higher education in Britain in the nineteenth century in the context of its challenge to society's dedication to separate spheres for males and females and to the existent ideal of womanhood. "The book concentrates on the opposition to higher education for women. It untangles the threads of opponents' arguments in order to show how serious a threat the movement of higher education was to the ideal of womanhood" (pp. 11–12). The book's nine chapters deal with the politics of aspiration, education for the middle classes, education and the ideal of womanhood, women and the economy, women's intellectual capacity, education and sex, religion and women's education, the ideal of womanhood confronting reality, and the opposition's influence on higher education.

Burstyn does not break new ground. In a concise and effective manner she brings fresh information to support well-established viewpoints; at other points in her study she summarizes material and at times makes perceptive comments upon assessments and conclusions that have appeared in monographs and articles in scholarly journals. Her ample footnotes are a rich source of information. Burstyn argues women succeeded in gaining access to higher education in the later years of the nineteenth century because men were less capable economically of caring for them. She maintains that although women's sphere changed due to the drive toward professionalism, the middle class was not convinced ideologically that its ideal of womanhood was in error.

Burstyn comments upon certain topics briefly, with generalizations that give a rigidity to her assessments. She writes that "the right of women to a liberal education for the sake of their development as individuals, a theme well developed in the twentieth century, gained little support in the nineteenth" (p. 139). On the contrary, a corps of women educators and advocates developed for middle-class women an ideology and curricula offerings at the secondary and advanced educational levels based on the value of liberal learning to development of the

female intellect, family relationships, and community life. Maria Shirreff Grey and Emily Shirreff wrote a number of works on that theme. The sisters were founders of the Girls Public Day School Company, whose more than thirty affiliated schools stressed the value of liberal learning for women. The North London Collegiate School of Miss Buss and Cheltenham College of Miss Beale were of a similar cast. A large number of middle-class families sent their daughters to these institutions. Indeed, female secondary education in the later years of the century brought to all of British education a new respect and a more imaginative approach to the teaching of what Miss Shirreff called the culture of the imagination: literature, art, and music.

Burstyn touches briefly upon the working-class woman and the ideal of womanhood, suggesting that reformers' interest in limiting the employment of working-class women, especially married women, reflected middle-class ideals about woman's proper place. The question arises: what was the working-class woman's reaction? The working-class woman and the ideal of womanhood need further exploration.

EDWARD W. ELLSWORTH
Wheelock College

JOHN S. HURT. *Elementary Schooling and the Working Classes, 1860–1918*. (Studies in Social History.) Buffalo: University of Toronto Press. 1979. Pp. x, 241. \$20.00.

The main lines of central government policy concerning the education of the working class in England in the second half of the nineteenth century are by now fairly clearly established, and much interesting work is in progress on those equally important bodies, the local and quasi-local authorities. What we know least about are the attitudes of the consumers, the children and their parents. Despite the promise of his title and early pages, John S. Hurt does not add much to our knowledge here. His biggest single bloc of sources is a familiar one—parliamentary papers—and he is vulnerable to the criticism that he levels at others, of overdependence on the views of middle-class observers. There are times, too, when his discussion of middle-class reporting of working-class attitudes, reactions, and behavior becomes shrilly polemical, sliding into an argument about class oppression. This is a perfectly defensible interpretation of elementary education in nineteenth-century England, and Richard Johnson has shown us with what force and subtlety it can be developed. But if such an interpretation is to be sustained, it needs a far more detailed and incisive discussion of the class structure and of the attitudes of different social and occupational groups

to schooling than Hurt provides. Comments like “the closer families were to the new forces of nineteenth-century industrialized society, the readier they were to spend money on education” (p. 32) are neither adequate nor, indeed, accurate.

Looseness of conceptual structure is matched by looseness of organization. Discussion of school attendance sprawls through chapters 2 and 3 and turns up again from time to time subsequently. And the terminal dates of 1860 and 1918 remain a puzzle. There are odd items of information about the situation on the eve of the 1870 Act, but they all make points that could perfectly well be made with material from the 1870s relating to the problems of making that Act in any sense “work.” At the other end, there is virtually no material later than 1914, and Hurt does not address himself at all to the impact of war on the developing school and welfare system.

GILLIAN SUTHERLAND
Newnham College,
Cambridge

FRANK M. TURNER. *The Greek Heritage in Victorian Britain*. New Haven: Yale University Press. 1981. Pp. xiv, 461. \$30.00.

Oscar Wilde claimed that Wordsworth only found in the stones of the Lake District sermons he had previously hidden there himself. Now Frank M. Turner convincingly demonstrates that the Victorians treated ancient Greek philosophy, history, and literature much as Wordsworth had the surroundings of Rydal Mount. Turner's discussion of the varieties of Victorian Hellenism in chapter 2 of *The Greek Heritage in Victorian Britain* relates how nineteenth-century Hellenists took one of three approaches to the ancient past: they presented an idealized version of Athenian life in the fifth century B.C.; used ancient Greece as a club with which to attack realistic or bourgeois tendencies in the arts; or, having abandoned a unitary conception of Greek civilization, posited a dynamic or evolutionary Hellenism that could serve as an ideal, a pole star, for modern times. The next chapter, which points out that Victorian students of Greek religion and myth spurned both the symbolical and mystical interpretations of the Renaissance and various rationalist ones of the Enlightenment, again traces attempts to find solutions to contemporary problems in the ancient past. Chapter 4, “The Reading of Homer,” demonstrates that *The Iliad* and *The Odyssey* were treated as surrogates for the Bible. In England the battles against German higher criticism of the scriptures were first fought against those who applied similar methods to the Homeric poems.

In contrast to these chapters, which skillfully

demonstrate the theological, philosophical, and spiritual dimensions of Victorian writings about classical Greece, chapter 5, "The Debate over the Athenian Constitution," concentrates instead upon political theory. Turner reveals one historian after another gamely attempting to attack or defend democracy with the materials of Greek history. This same, often desperate search for roots, guides, and moorings appears again in chapter 6, "Socrates and the Sophists," which relates how the usual Victorian urge to find—or create—a prescriptive past produced a liberal Anglican version of Socrates, in which he appears divinely inspired; a heroically skeptical rationalist version; and, finally, a mystical one that would provide solace or inspiration for those who had fallen away from Christian belief. After an excellent chapter on Aristotle's *Nicomachean Ethics*, which conveys much of importance about intellectual life in the Victorian university, the closing discussion of the period's Platonic revival reveals the philosopher in his various Victorian guises as a transcendentalist prophet, a skeptical epistemologist, and an idealistic surrogate for Christian social and political values.

As Turner explains, he does not discuss allusions to Greek myth and culture in Victorian literature (or art), the role of the classics in education of the period, or the history of linguistic and philological studies of classical Greece. Nonetheless, although he does not propose to provide a full history of Victorian intellectual life, his always clear, concise discussions permit us to perceive the texture, the interweaving, the complete *context* of many Victorian issues that one often encounters far less effectively presented. Indeed, one of the most impressive qualities of *The Greek Heritage in Victorian Britain* is the way Turner frequently introduces discussions of individual works with brief capsule biographies of their authors that effectively relate the specific works to entire careers, intellectual movements, and broad tendencies. Taking those authors with whom I am most familiar—Ruskin, Carlyle, and Arnold—I find that Turner is one of those rare cultural or literary historians who are not led astray by the polemical abilities of these sages, for he never takes them out of context and his summaries always seem right on target.

This well-conceived and well-executed study sheds new light on familiar figures, does much to attract us to those who are less well known, and places them all in clear relation to each other. In brief, *The Greek Heritage in Victorian Britain*, which makes major contributions to our understanding of the intellectual life of the last century, will be of great interest to students of Victorian art, literature, and ideas in both England and America.

GEORGE P. LANDOW
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BRIAN JENKINS. *Britain and the War for the Union*. Volume 2. Montreal: McGill-Queen's University Press. 1980. Pp. 470. \$26.50.

The second volume of Brian Jenkins's *Britain and the War for the Union* concludes a major study of an important and complicated subject. While it offers nothing startlingly new in evidence or interpretation, it is a solid, well-written synthesis based on thorough archival research. It should appeal to the general reader as well as the specialist.

Jenkins continues the story of Anglo-American relations during the American Civil War from the spring of 1862 to the end of the war in 1865. There is less attention paid to developments in Richmond and Ottawa in this volume, but Jenkins brings into sharper focus than other studies the complexities facing the Palmerston government in its attempt to define its course. The issues of "King Cotton" versus "King Corn"; insecurity regarding Canada; domestic politics, including slavery and Lancashire unemployment; turmoil in Europe involving Poland and Schleswig-Holstein; and, especially, the threat seemingly posed by Napoleon III are all treated judiciously. Due attention is paid to public opinion on both sides of the Atlantic as well as to the attempts by both belligerents to mold opinion in Britain.

Jenkins carefully analyzes the abortive British mediation effort in the fall of 1862 and dexterously traverses the swampland of international and domestic law underlying the issue of Confederate ships built in Britain. A point that Jenkins correctly emphasizes is the tremendous hostility toward Britain that developed in both North and South. In their American policy, the British were damned if they did and damned if they didn't. Fortunately for the Union and, arguably, also for Britain, the people who really mattered kept their heads. Seward, in particular, emerges as a model of restraint.

The main deficiency of this book is its mechanics. Responding to criticism of the first volume, Jenkins employs omnibus notes in this one but only succeeds in exacerbating the problem. The notes do not always indicate exactly the source of the direct quotations, and no indication is made of the relative merits of the sources cited. Often there is one note for an entire paragraph in which several direct quotations from different sources appear, and sometimes one must struggle to determine who was writing to whom. Further, his novel use of *ibid.* to refer to the same correspondents even when the source is in a different location from the preceding reference serves to confuse and, ultimately, to irritate. Finally, there is no bibliography. The first volume has a bibliographical essay, but it does not cover all the material cited in this volume. A simple concluding list of references would have made the book much more valuable to scholars.

This is, nevertheless, an important book. In his introduction, Jenkins modestly disclaims any intention of supplanting E. D. Adams's classic study. He does not. Instead he has produced a work that deserves a respectable place on the shelf next to Adams. It will stand as this generation's definitive assessment of Anglo-American relations during the Civil War.

DAVID F. KREIN
Scott Community College
Bettendorf, Iowa

E. H. HUNT. *British Labour History, 1815–1914*. London: Weidenfeld and Nicolson. 1981. Pp. xii, 428. Cloth £18.50, paper £8.95.

E. H. Hunt intends his *British Labour History, 1815–1914* both as "an introductory textbook that treats British labour history as a whole" and as an attempt to rectify the lack of attention given, in labor history, to the findings of historical demographers, business historians, and labor economists (p. xi). Part I deals with the labor market, incomes, and consumption, and includes chapters on the labor force and employment, demography, wages and living standards, and poverty and population distribution. It is here that Hunt succeeds best. He portrays graphically the causes of mortality (p. 42 and following) and has collated interesting information on housing.

Part 2 views working-class movements from the standpoints of government, the employing classes, and current economic history ideas. There is little sense of the turmoil of technological and occupational change, especially in the first half of the period under study. Nor does Hunt show how these changes—allied to a growing overall labor surplus, despite local and temporary shortages—enabled the socially strongest group, British men, to oust other, easily identifiable groups from work that they wanted. Like others he places an alarming reliance on census figures of numbers of female workers, despite the well-known tendency—especially during the changeover from domestic to factory industry—for women to be given their family and not their work status. There are specific examples of underestimates, for example, in the 1841 census in nail, glove, and ribbon manufacture. Hunt is perfunctory on women's role as an integral part of the manufacturing labor force other than in textiles, when it is self-evident that their work was essential in potteries, brickyards, and the vast range of small metal trades of the West Midlands. He gives scant attention to the importance of this region for the development of engines, arms, and domestic metalware, all associated with the rising standard of living that he describes.

Hunt seems to ignore the elementary point that no amount of productivity will raise "wages" unless there are markets in which to sell the products (p. 109). It was such a conjunction, together with access to raw materials both abroad and at home, that gave British working people for a short time higher earnings than those attained on the Continent. Hunt appears to believe that labor historians can avoid political commitment but, inevitably, his own choice of material makes his views plain. His welcome references to social hierarchy are not used to explain collaboration between employers and employed in certain areas. And he fails to see that the appearance of an organized labor movement, however fragmented, as a political force by 1914 was itself, in the eyes of propertied people, a revolution. Although his book may help students at British universities in the next five to ten years, it covers neglected aspects rather than British labor history as a whole.

SHEILA LEWENHAK
London, England

RICHARD JAY. *Joseph Chamberlain: A Political Study*. New York: Clarendon Press of Oxford University Press. 1981. Pp. ix, 383. \$49.50.

Lord Salisbury once observed, when a Chamberlain speech was due, "you may look out for squalls." It was so. Joseph Chamberlain aroused fierce partisan controversy and extremes of devotion and hatred that continued long after his death. Historians have extended the debate, creating their own squalls, by offering radically contrasting evaluations of Chamberlain and the events of his career.

As Richard Jay notes, judicious comment emerges with difficulty from such a "plethora of views." Yet he succeeds, providing a clear narrative thread through the complex disputes of Chamberlain's public career and, in a splendid concluding essay, giving a balanced and perceptive assessment of the man's contribution to the politics of his day. The narrative flags in places, especially for the 1890s, but only the final chapter, devoted to the years 1906–14, disappoints. The account seems inadequate after Alan Sykes's study, *Tariff Reform in British Politics, 1903–1913* (1979), which so clearly explains why Chamberlain's last great crusade failed and became little more than a defense of limited class interests.

Jay warns against looking for perfect consistency in Chamberlain's career. He was constant only in style. But Chamberlain was more than a dramatic careerist, more than Gladstone's "prince of opportunists." At his worst, says Jay, he reflected many of the lowest commonplace prejudices of his day; chauvinism, both national and sexual; the cult of

racial superiority; and the cultural philistinism of the English middle classes. At his best he challenged the complacency of the dominant ideologies, bringing into the forefront of politics the realities of urban industrial life and identifying the strains and tensions that threatened the foundations of Britain's domestic and imperial stability. Chamberlain's legislative record was slim and his great crusades were failures, but he offered "genuine alternatives to the eventual course of British politics" (p. 351).

Chamberlain was not overly nice about methods; throughout his life he was capable of sharp practice, but he was clever and covered his tracks well. Jay discusses in detail two episodes that reflected on Chamberlain's honor. The first was the Crawford divorce case, which ruined the career of Charles Dilke, a friend, ally, and political rival. In this affair did Chamberlain conspire to eliminate a competitor? Jay concludes that the charge is not proven and is probably unprovable but that Chamberlain was the kind of man who could excite against him the sort of charges that were leveled in connection with the Dilke case.

The second episode was the Jameson Raid and Chamberlain's fight to keep a parliamentary inquiry from implicating him in planning the invasion. Chamberlain had prior knowledge of the operation but escaped exposure and censure. Incriminating evidence disappeared. Of the coverup Jay writes, "It was a superb, if disgraceful, exercise in political survival" (p. 207). These episodes help to explain Salisbury's observation that Gladstone, though very much hated, was also very much loved, but "Does anyone love Mr Chamberlain?"

J. A. THOMPSON
University of Kentucky

MAX EGREMONT. *Balfour: A Life of Arthur James Balfour*. London: Collins. 1980. Pp. 391. £12.95.

Max Egremont is rash enough to ask, in the introduction to this book, "Why then embark on another life of Arthur Balfour?" Certainly Balfour has already received what is becoming the standard treatment for a British prime minister: official life, not too satisfactory (Dugdale); recent life by talented nonprofessional (Kenneth Young); and more scholarly if perhaps less appealing life by academic (Zebel). This is very much the way Asquith has been handled, and it is close enough to what has been done for Lloyd George, Campbell-Bannerman, and Rosebery. What is there to suggest that Balfour needs more attention, or that historical understanding will benefit from a new biography? At any rate, the results do not suggest that Egremont has spent his time in the most rewarding way. Even if he insisted on confining himself to his own family, he could stick to prime ministers and write about the

rather unapproachable Lord Salisbury, or he could try another dual biography like his interesting study of Wyndham and Blunt, *The Cousins*, and write about Lord Hugh and Lord Robert Cecil.

Obviously no reviewer would press this sort of good advice either on a writer who showed no capacity to write a satisfactory book, or on one who had just done so. Egremont has run into trouble here because the book adds relatively little in material or in interpretation to what has been said in previous studies. This is not just a matter of the full-length biographies; the account of the 1903 cabinet split (pp. 176–84) does not show a full understanding of the complex argument put forward by Gollin in *Balfour's Burden*. The papers of J. S. Sandars are the only new collection that has been used, and they would be more useful in the hands of someone more deeply interested than Egremont in the mechanics of party management.

Some aspects of the book that may strike a historian as deficiencies are the result of the author's desire not to bore his readers by going into technical details, a natural (and a proper) attitude when writing for a nonprofessional readership. This still leaves the question of whether Egremont's interpretation is one that can comfortably be handled in such a way. For instance, he says that Balfour's "triumphs were generally administrative rather than political." This is a perfectly arguable point, but if it is to be made, Egremont ought to devote more care to explaining successes like the 1902 Education Act. At the level of detail, he ought not to say "higher education" when he means "secondary education," and at the level of administration he ought to say something, however brief, about the role of county councils under the act.

In many ways the activities of conservative statesmen are harder to explain than those of radicals; there is usually no easily explicable cause for which to fight, and very often there is nothing more exciting than a series of rearguard actions to record. Egremont reasonably enough sees Balfour as an alert Conservative, defending the existing state of affairs with great dexterity, but he does not devote enough time or emotional energy to explaining why this defense was desirable and necessary. Kenneth Young did make it clear what Balfour was fighting for; in this book he at times sounds like a civil servant with an active and enjoyable social life. Balfour deserves more than that, and all the evidence suggests that the author can rise to other subjects better than he has to this one.

TREVOR LLOYD
University of Toronto

MARTIN J. WIENER. *English Culture and the Decline of the Industrial Spirit, 1850–1980*. New York: Cambridge University Press. 1981. Pp. xi, 217. \$15.95.

Central to the argument of Martin J. Wiener's *English Culture and the Decline of the Industrial Spirit, 1850–1980* is the author's conviction that "ideas are indeed 'real' and have consequences" (p. x). Wiener is equally certain that a study such as his can exclusively focus on middle- and upper-class cultural values because "the values of the directing strata . . . tend to permeate society as a whole and to take on the color of national values" (p. 5). It is clear from the start that this is an unabashed exercise in old-fashioned elitist cultural history.

With these guiding assumptions, Wiener seeks to explain what he perceives as the cultural counter-revolution of the mid-nineteenth century. Just at the moment of their apparent triumph at the Crystal Palace, the cult of industry, aggressive capitalism, and dynamic entrepreneurship began losing ground to a different vision of England that harked back to earlier times. This national self-image emphasized altogether contrasting values: stability, order, tranquillity, harmony instead of competition, quality instead of quantity. It was a vision of England that depended heavily on the pastoral day-dream, or rural myth, that has sounded a persistent chord in anti-industrial opinion since the birth of industry.

Wiener traces the enduring strength of that day-dream down to the present. He rightly finds it embodied in decades of social criticism from the left and right alike; he charts its place in public schools and ancient universities where, as the cult of the landed gentleman, it molded generations of statesmen, civil servants, professionals, financiers, and also industrialists. He reiterates that because of this cultural ideal, which accords no social status to the businessman, the energies of England's managerial class have been directed away from industrial expansion, innovation, and the pursuit of profit. Instead of maintaining Britain in the forefront of economic growth, they have allowed its industrial spirit to sag while they emulated the gentry.

Although this summary simplifies Wiener's argument, it does not distort it. Throughout the book the principal line of reasoning returns to the same contention: to understand Britain's dramatic industrial decline, we must look to attitudes deeply ingrained in the middle and upper classes. Although Wiener pays occasional lip service to the importance of economic forces, he obviously finds them far less significant than the cause that he investigates. Both at the beginning and end of his book, Wiener specifically challenges Eric Hobsbawm's injunction that economic phenomena ought to be explained in economic terms and rejects the Marxist approach to the question.

Wiener's own approach remains profoundly unsatisfactory. What can one make of a theory of industrial decline that virtually ignores the trade

unions and attempts no inquiry into the culture of the factory itself? The pastoral-industrial dichotomy, furthermore, is painted with such heavy-handed strokes that, for example, the contest between A. J. Balfour and Joseph Chamberlain for control of the Conservative party is misleadingly reduced to a fight between the new industrialist and the scion of a great landed family. Finally, the heavy reliance on secondary sources also diminishes the usefulness of the study, work of synthesis though it admittedly is.

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OLAVI HOVI. *The Baltic Area in British Policy, 1918–1921*. Volume 1, *From the Compiègne Armistice to the Implementation of the Versailles Treaty, 11. 11. 1918–20. 1. 1920*. (Studia Historica, number 11.) Helsinki: Finnish Historical Society. 1980. Pp. 231.

Olavi Hovi's doctoral dissertation is an attempt to present a detailed study of British policy in the Baltic region from the end of World War I to immediately after the Paris Peace Conference. Based on extensive research in British and German Federal Republic archives and, to a lesser extent, on Swedish, Danish, and Finnish archival materials and an impressive array of published primary and secondary sources, Hovi's work is a coherent narrative and assessment of British efforts to maintain their influence and to protect their interests in the Baltic area. Beginning with a review of the major features and objectives of British foreign policy in the Baltic to the armistice in 1918, Hovi proceeds to examine Britain's use of naval forces and the deployment of German troops against the Bolshevik regime (December 1918–February 1919), the "re-emergence" of the German problem in conjunction with the Bolshevik menace in the Baltic (February–June 1919), and finally Britain's attempts to deal with German and Russian threats to British interests in the Baltic (July 1919–January 1920).

Hovi discerns three distinct periods in Britain's Baltic policy. During the first period (October 1918–February 1919), the War Cabinet moved to check the flow of Bolshevism into the East European territories conquered by Germany during the war and to develop long-term objectives for the Baltic area. These objectives were to internationalize the Danish straits and to support the independence and unity of Finland, Estonia, Latvia, Lithuania, and Poland as barriers between Germany and Russia. In the second phase (February–June 1919), the Bolshevik danger increased and, with German *Freikorps* troops in Latvia, posed a serious threat to British interests in the Baltic. During April and May, as the Russian White armies seemed on the verge of success, Britain continued to support the indepen-

dence of the Baltic states and encouraged them to arrange settlements with the future Russian government. The third period (July 1919–January 1920) was marked by British efforts to stabilize the Baltic by international agreement and, as the Bolsheviks triumphed in the Civil War, to press the Baltic nations to define their relationship with Russia. Indeed, London refused to accord *de jure* recognition to the Baltic states until they had rid themselves of German “volunteers” and White army forces. Finally, by the end of 1919, Britain accepted the facts that German and Russian influence could not be excluded from the Baltic region and that it might be impossible to prevent an entente between revolutionary Russia and Germany.

The surprising accord on Baltic policy between the Foreign Office, the Admiralty, and the War Office enabled Britain to facilitate “the . . . stabilization of the status of the three Baltic states” (p. 211). Thus, in view of what limited power Britain could exert and what opportunities it utilized, the results of its Baltic policy during 1918–20 were quite positive.

Although this interesting monograph is enhanced by an excellent bibliography, it is flawed by an awkward English translation, typographical errors, and an inadequate index.

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MICHAEL L. DOCKRILL and J. DOUGLAS GOOLD. *Peace without Promise: Britain and the Peace Conferences, 1919–23*. Hamden, Conn.: Archon. 1981. Pp. 287. \$25.00.

Given the wealth of recent specialist literature on Britain's role in the peacemaking after the Great War, the moment is opportune for a treatment that draws on the best of this rich corpus and weaves it into a convenient and convincing synthesis. If the authors can, as well, add detail from their own research and reflection, so much the better. Michael L. Dockrill and J. Douglas Goold are well qualified for the task, each being thoroughly familiar with both the primary sources and the secondary literature. Their book stands as a thoroughly lucid, economical, and often insightful synthesis of the best published work on the major phases of Britain's role in the peacemaking.

After a brief introduction on the development of war aims and British preparations for the peace conference, the authors examine the making of the German settlement. This is well-trodden ground and on the territorial settlement there is not much new to be said beyond previous studies like Harold Nelson's durable *Land and Power* (1963). On the topics of reparations, colonies, and the League of Nations, however, the authors effectively bring for-

ward the results of recent studies that revise the older treatments.

The reader's interest will quicken when the treatise turns to the less familiar terrain of the East European settlement. But it is in the final two chapters dealing with the Middle Eastern and Turkish settlements that the authors fully engage the reader's attention, offering analysis and synthesis rich in insight, provocative in commentary, and elegant in style. Here we have diplomatic history shown to full advantage as the authors exhibit a familiarity with the substance and processes of British policymaking that transcends, in places, the best of the published literature. Readers are offered perhaps the clearest brief analysis to date of the complex, contradictory, and emotionally charged commitments and interests guiding British policy vis-à-vis the Turks, the Arabs, and the principal Allies. The confusion in British policy making and the baleful influence of Lloyd George that led to the Smyrna and Chanak debacles is portrayed dramatically and unsparingly. Curzon is given full credit for the effort of diplomatic reconstruction leading to the Lausanne settlement, the most durable of all the postwar settlements.

The authors are clear that they are writing diplomatic history. And, if they have offered us an excellent example of this genre, the reader may regret that they have made little effort to locate their studies in the major political, economic, social, or ideological transformations of modernity that the Great War both incorporated and massively accelerated. The authors stick close to the official record and exhibit a tendency to stress the inevitability of the British role even when the consequences were disastrous. For the most part there is a sympathy with the Foreign Office and professional advisors, whose general impotence is much regretted. Nevertheless, scholars will find this admirably concise and informative study of major usefulness as a reliable guide to the principal features of the British role in the peacemaking of 1918–23.

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JANE LEWIS. *The Politics of Motherhood: Child and Maternal Welfare in England, 1900–1939*. London: Croom Helm or McGill-Queen's University Press, Montreal. 1980. Pp. 235. \$27.95.

Jane Lewis's subject is the emergence of social policies purportedly aimed at improving the welfare of children and mothers in England between 1900 and 1939. Purportedly because, as she argues, many of the policy makers were interested in such plans only to the extent that they might improve England's racial stock or benefit the British empire's national power and prestige. Lewis's thesis is, how-

ever, that the policies themselves took form as a result of the interactions among policy makers, health care providers, and mothers.

Of special significance is the emphasis that Lewis places on the participation of the mothers ("clients," to use her designation) in the formulation of programs affecting children and mothers. Generally, English social policy in the nineteenth and early twentieth centuries emerged out of a paternalistic system in which the needs and desires of clients were largely ignored. (One only has to reflect upon the history of the nineteenth-century poor law or slum legislation in order to understand the point.) Lewis argues, however, that the situation was different with respect to the politics of motherhood. Because some mothers were articulate, organized, and determined, they could and did act as pressure groups in their efforts to influence the direction and shape of welfare policies.

While her point of view is aptly defended and while she provides evidence to show that mothers were not helpless or disorganized in defense of their own interests, Lewis's work raises two important issues that need fuller examination. The first is her tendency to treat mothers as a generic category without reference to the English class structure. If one examines the various women or organizations with which she concerns herself, it is apparent that they were affluent and well educated. Without saying so, Lewis is really discussing the politics of middle-class motherhood rather than motherhood in general. It is a distinction worth making and considering.

Although Lewis demonstrates admirably the efforts that (middle-class) women made in pursuit of their interests, she ignores an important question. Did their efforts make a difference? In the preface she provides a tantalizing hint that their energy was wasted but does not pursue the matter further. The lacuna is unfortunate. An attempt to answer the questions would have allowed the reader to understand more fully the impact of the mothers' lobbying activities. Also, the attempt would have suggested to what degree the mothers' endeavors helped to bind them into organizations that persisted in time beyond the chronological confines of her book.

Despite these two reservations, I think that Lewis's work is an important investigation into the history of motherhood. By using fresh evidence, she adds to the perspectives contained in other scholarly works about birth control, midwifery, eugenics, and the ideology of motherhood. Her chapter on economic assistance gives warranted recognition to the ideas and work of Eleanor Rathbone.

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FRANK HONIGSBAUM. *The Division in British Medicine: A History of the Separation of General Practice from Hospital Care, 1911-1968*. New York: St. Martin's Press. 1979. Pp. xvi, 445. \$27.50.

The product of many years of research in primary sources, this is a detailed, provocative study of Britain's medical system and the separation of general practice from hospital care between 1911 and 1968. Frank Honigsbaum, who holds an M.B.A. from Harvard Business School, was formerly research director of an American labor union and has published elsewhere on the history of general practice in England.

Unlike the organization of medical delivery in the majority of the industrialized nations, England's general practitioners (G.P.s) are the first point of entry into the medical care system. Yet the British G.P. plays almost no role in hospital care; this is assumed by the consultant-specialist. Honigsbaum examines the historical process by which G.P.s were separated from the hospital world, tracing how medical politics and government intervention deepened what he interprets as an earlier class division between G.P.s and hospital-based specialists. Many British physicians will have difficulty in concurring with Honigsbaum's (undocumented) view that "G.P.s tended to come from a lower class than consultants" (p. 318), with the class struggle model itself, and with the assumption that the G.P.-consultant division is necessarily deleterious to medical care.

The study opens with the formation of the panel doctor system following passage of the Lloyd George Insurance Act in 1911, which provided G.P. (but not hospital or specialist) services for 15 million working-class Britons. Honigsbaum discusses the role of the Medical Practitioners Union and the failure to implement the concept of health centers recommended in the Dawson Report. Noting the efforts of some leaders of the Medical Practitioners Union, from 1921 on, to open hospital doors to G.P.s, Honigsbaum attributes their continued exclusion to the antagonism of consultants and to an "inferiority complex" that G.P.s developed in the face of advancing specialism.

There are interesting accounts of the antivivisectionist influence on medical practice, of trade union politics, of the Socialist Medical Association, and of the narrowing range in the 1930s of care traditionally provided by general practitioners. Honigsbaum also explores the stirrings of antisemitism that accompanied England's absorption of 3,000 medical refugees by the end of World War II. When the concentration camp atrocities were revealed in British newspapers after 1946, the Medical Practitioners Union reacted strongly against antisemitism. The

period 1948–68, covering twenty years of the National Health Service, is treated only briefly.

This is a valuable addition to the studies on recent medical practice and hospital care in Britain by Rosemary Stevens and Brian Abel-Smith. It is broadly based on government documents and the British medical press, as well as on private papers. The rich breadth of source materials covered has posed problems of synthesis and readability that are not wholly resolved. Honigsbaum has tried to assist the reader through frequent subtitles and useful biographical notes on major figures. The printing, paper, and format are excellent.

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National Library of Medicine

ALAN D. GILBERT. *The Making of Post-Christian Britain: A History of the Secularization of Modern Society*. New York: Longman. 1980. Pp. xv, 173. £8.95.

MAURICE COWLING. *Religion and Public Doctrine in Modern England*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1980. Pp. xxiv, 475. \$49.50.

Both of these books are written in the light of their authors' convictions about the condition of Christianity in Britain. For Alan D. Gilbert, Britain is a "post-Christian society" in which Christianity has become marginal, and he is concerned to explain how and why this has happened. In Maurice Cowling's view, "post- or anti-Christian doctrine" has gained ascendancy in England and much Anglicanism in particular has sold out to superficial, humanistic liberalism. Gilbert sees "the social and psychological pressures of modernization" continuing while Protestantism survives in sectarian forms, a fate that may overtake Roman Catholicism. Cowling's view of the future is not altogether dissimilar: he hopes for a revival of a "Christian conservatism" that, even if it cannot dominate, will offer sharp dissent.

Gilbert, well known for his *Religion and Society in Industrial England* (1976), describes his latest book as concerned with "the relationship in modern Britain between the decline of religion and the emergence of a complex urban-industrial society" (p. 15). (For both Gilbert and Cowling "religion" means Christianity.) The book falls into three parts. The first of these, theoretical in character, defines "religion" and "secularisation" and sketches the secularizing process. In the second part the emergence of "post-Christian Britain" is described, while the final section notes religious responses and concludes with a glance at future directions.

The Making of Post-Christian Britain may be regarded as updating the relevant sections of Bryan Wilson's *Religion in Secular Society* (1966). Those familiar

with recent literature will not find anything especially new in Gilbert's account. It is, however, clear, coherent, and supplemented by a useful bibliographical note and references. The account is not without limitations, but perhaps brevity is largely responsible. Judgments on theological developments are sometimes misleading: that Schleiermacher "tried to evolve a faith so essentially subjective as to resign 'all claims on anything that belongs either to science or morality'" (pp. 119–21) is not the happiest statement of his aims. The thesis that ecumenism is born of weakness and constitutes "lateral" growth is qualified to some extent; some may think not sufficiently so. On the whole, however, given the limitation of space, Gilbert has provided an admirably succinct essay that may be read on its particular subject and as a source of fertile comparisons.

For several reasons Cowling's book is more difficult to estimate. For one thing, it is the first volume of a much larger study. What we have before us is an account of how the author got where he is, from whence he proposes to write about "public doctrine" in England since 1840. The expression "public doctrine" is unusual. By it Cowling means "the assumptions that constitute the framework within which teaching, writing and public action" (p. xi) are conducted. He believes that those who have contributed to "public doctrine" have, whether they realized it or not, been answering questions about what the English should believe about Christianity, its role, and its implications for public affairs.

The choice of the figures discussed appears to be determined not by their public or even (in some cases) their academic importance, but by the fact that they influenced Maurice Cowling! Only some such rationale could link so diverse a collection. First up are Whitehead and Toynbee, to whom Cowling initially responded warmly and then, in the case of Toynbee, with critical distaste. The cure seems to have been effected by a strong dose of "Christian conservatism" administered by "Three Anglican Reactionaries": Charles Smyth, T. S. Eliot, and David Knowles (probably the best known). Further on the way to evolving his own standpoint, Cowling was influenced, both positively and negatively, by R. G. Collingwood, Herbert Butterfield, Michael Oakeshott, and Winston Churchill. We draw even closer to the author's position in the final section, where Eli Kedourie, Evelyn Waugh, the third marquess of Salisbury, and some present members of the Cambridge history faculty rub shoulders. The last figure to be considered is Edward Norman, and one suspects that, despite the criticisms offered, it is this type of pungent Christian conservatism to which Cowling is attracted and with which he will advance on "English versions of modernity."

There is much that is unexpected about this

volume: the angle of vision enables Cowling to see some things and miss others, even when the target is as ample as T. S. Eliot. Either way there are some useful analyses and clever remarks. What to make of it all we shall not quite know until Cowling raises the building for which this book has cleared the ground. Considered in its own terms the book has interest as an account of how one English historian has come to sympathize with "Christian conservatism." Since there seems to be a good deal of this about, Cowling's book has usefulness apart from the larger and slightly mysterious purpose intended by its author.

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FRANCES J. SHAW. *The Northern and Western Islands of Scotland: Their Economy and Society in the Seventeenth Century*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. ix, 270. \$39.00.

To examine the economy and society of all the main Scottish islands during the seventeenth century is an ambitious undertaking. Their similarities, other than a remoteness from the seat of government in Edinburgh, are not striking. Orkney and Shetland retained a strong Norse legacy well after formal Norse rule ended in 1468–69, whereas the predominant influence shaping the Western Isles was Celtic. Not surprisingly, the structure of landholding and the nature of wealth and status differed enormously between the Hebrides and the Northern Isles. The Hebridean chiefs enjoyed a vastly different life style from the more modest large landowners of Orkney and Shetland. Ensclosed in substantial castles like Dunvegan (Skye) or Lochbuie (Mull), venerated by their followers, and surrounded by considerable households, the chiefs were proprietors of great estates let to a diversity of tacksmen, the more important of whom were usually kinsmen. The tacksmen similarly let to innumerable subtenants, and the whole pyramid was held together not merely by rents but also by the ancient social cement of elaborate military, administrative, and personal services. In the Northern Isles, by contrast, there was absolutely no parallel to the Hebridean chief, while at all levels the persistence of much allodial landholding had meant the fragmentation of estates. The Northern Isles, too, possessed nothing comparable to the growing cattle export of the Western Isles (and Highlands) that, in their turn, knew little of the seaboard trade and ships' virtualing practiced by Orkney and Shetland generally and Kirkwall in particular. Fishing, of genuine significance to the economy of the Orcadian and Shetlander, was relatively neglected by the Western Islander, although there were exceptions.

Thus, much of Frances J. Shaw's brief is to itemize differences of emphasis in agriculture, craft, trade, and social structure, a duty she discharges with painstaking scholarship. The danger of too static a picture emerging is largely, though not entirely, offset by the author's analysis of the Scottish crown's attempt to exert and increase its influence over all the island areas. For example, James VI abrogated Norse laws in the Northern Isles in 1611, and in 1597 and 1616 encouraged enactments to tame Highland custom and limit chiefly retinues. Over the century there was a distinct trend toward an erosion of the older values and relationships (regretted by Gaelic bards), although, as Shaw remarks, the traditions of paternalism and loyalty died hard. Adoption of more sophisticated tastes was one element bringing financial strain to the Hebridean landowners who consequently sought a deeper involvement with commercialism, especially the cattle trade, and with leases based on economic as well as strategic considerations. One would have welcomed a fuller discussion of this process, although in fairness the author was clearly concerned to cover a wide range of topics within a relatively modest space. In this aim she has been successful, and her work will long remain a rich quarry of fascinating data and stimulating comparisons in an area of both geographical and historical complexity.

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J. RUSSELL MAJOR. *Representative Government in Early Modern France*. (Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, number 63.) New Haven: Yale University Press. 1980. Pp. xiv, 731. \$45.00.

"Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions" have often contained more than a hint of the Whig interpretation of history. The present massive contribution to this series constitutes the culmination of J. Russell Major's long endeavor to combat the reverse trend among historians of early modern France, namely the tendency to antedate the growth of absolutism and to undervalue the vitality of representative bodies. His primary concern is the history of the provincial estates, which he regards as the determining element in French government in the fifteenth and sixteenth centuries. Despite its title, the book takes little account of national estates general and assemblies of notables and has an even more cursory treatment of local assemblies. In the first part, Major redefines his terms, brushes aside earlier criticism, and continues to defend his thesis that the French Renaissance

monarchy was popular, consultative, and decentralized. The second part discusses the origin and development of absolutism in the seventeenth century, and here Major's revisionism, which will doubtless excite an equal amount of controversy, seems fresher and far more persuasive than his account of Renaissance constitutionalism.

Archival sources concerning the provincial estates are so extensive that Major has been obliged to summarize particular monographs, although he has disagreed with their conclusions when they suggest decline rather than advance in the influence of the estates. He has also made skillful use of local archives at critical points. It is not impossible, as he suggests, that royal authority and the strength of provincial estates could sometimes grow in parallel, but tension over the control of taxation prevented identity of interest. Even so, one does not characterize a tiger by the stripes on its tail. In order to understand the nature of sixteenth-century government it is necessary to take as much account of the daily operations of the council, judicial and fiscal reforms, clientage, and the growth of venality of office, as of the powers of the estates. The sovereign courts in the provinces are discussed from time to time in this context, as also are the *trésoriers* and the *élus*, but it is the issues of tax consent and collection that dictate Major's conclusions. In this respect he occasionally indulges in special pleading. When the crown creates *élus* to collect taxation in the *pays d'états* under the Valois kings, Major assumes that this is a device to make the estates pay for their suppression; under the Bourbon kings such creations are usually seen as evidence of a desire to destroy the estates. Moreover, if the existence of *élus* is regarded as the vital factor, considerably less than half of Renaissance France contained provincial estates that could successfully defy the crown's fiscal demands.

Henry IV's preference for Sully over Bellièvre in 1604 is taken to be a crucial step in undermining representative bodies. The conflict between the two ministers, however, suggests more than the future relations of the crown and the estates. It involves the distinction between the executive and the judicial approach to government that may in the long run have been more significant in the development of absolutism. Major convincingly attacks the belief that Richelieu was the architect of absolute monarchy. Its designer was the devout and inflexible Michel de Marillac, who resumed the work of Sully and planned the systematic destruction of the estates. The cardinal is depicted as a greedy and ambitious manipulator, an "evil genius" (p. 451) who directed Louis XIII's attention away from domestic reform and converted his reign into "an unmitigated disaster for the mass of the French people" (p. 621). Even Richelieu's foreign policy is described as inept. Colbert, it is argued, would have

preferred Marillac's path, but once again the quest for glory frustrated a fully institutionalized absolutism. The provincial estates and *parlements* eventually recovered from Louis XIV's restrictions and reappeared as the focus of opposition.

While Major has taught us a great deal about the provincial estates, the broad inferences he has drawn about the nature of early modern French government remain controversial. His book concludes on the note of what might have been. Had Marillac triumphed over Richelieu, a paternalist absolutism might have postponed the Revolution. Had Henry IV reposed his trust in Bellièvre after the religious wars, Major's idealized Renaissance monarchy might have endured even longer.

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HERBERT H. ROWEN. *The King's State: Proprietary Dynasticism in Early Modern France*. New Brunswick: Rutgers University Press. 1980. Pp. viii, 232. \$19.50.

In this brief book, Herbert H. Rowen argues that the early modern French kings did not just hold an office: they also believed that the state was personal property belonging to the dynasty. There was, therefore, a conflict between what Rowen calls the "office theory" and the "property theory" of kingship, a conflict that was inherited from the Middle Ages and that lasted to the end of the monarchy. That the kingdom was considered the property of the kings is not a new idea, but no one has previously devoted so much attention to the topic, and no one has systematically faced the problems posed by the two conceptions of kingship.

Two major criticisms of the work are worth noting. The first is Rowen's failure to define his terms clearly—state, dynastic kingship, the domain, sovereignty, and feudalism, among others. He often confounds concepts that should be kept separate. The terms have juridical content and require precise definition. Here Rowen is not completely at fault, for most of the terminology has never been satisfactorily delineated; nevertheless, he should have devised working definitions and stuck to them. Second, the work does not treat adequately of the Middle Ages. The chapter devoted to that period is superficial and betrays the writer's reliance upon secondary sources that are not always reliable. (Rowen is clearly at his best in the period after 1650, for which he usually cites primary sources.) This is extremely unfortunate because an adequate understanding of the medieval background is essential to explain the modern phenomenon.

What in fact happened is that medieval kings were likely to think of themselves as possessing essentially personal rights (and not property in a geographical sense as Rowen and many other writ-

ers seem to suggest), that they developed an appropriate vocabulary, and that this remained in existence after the state began to be distinguished from the kingship from which it grew. In the age of lawyers after 1500, that vocabulary was eclipsed by abstract political concepts and the idea of kingship as office. With Louis XIV there was a revival of the older terminology, which was given new content that remained alive throughout the eighteenth century.

The subject is a slippery one, and he who treats of it must expect to fall occasionally. A quotation from Louis Charondas Le Caron's *Pandectes* (1596) illustrates well the difficulties: "Puis que l'estat de la France est gouverné par la puissance souveraine d'un seul, . . . il faut brievement traicter ce qui appartient à sa souveraineté, pour cognoistre en quoy consiste l'estat du droit public François" (p. 143). One hardly dares to translate the passage for fear of making it say something that it does not, and the same is true of many of Rowen's texts. The book raises problems well worth pondering, and, although one may have strong reservations, even the skeptic can hardly deny that this may prove to be a seminal work.

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ROHAN BUTLER. *Choiseul. Volume 1, Father and Son, 1719–1754*. New York: Clarendon Press of Oxford University Press. 1980. Pp. xxxv, 1,133. £48.00.

Biographies of Étienne-François, Duke de Choiseul, are surprisingly rare considering the importance of this chief minister of Louis XV during the twelve crucial years of the reign. This lacuna is on the way to being filled by the publication of the first volume of a planned three-volume biography by Rohan Butler. The initial volume covers the first half of the life of Choiseul, from his birth in 1719 to the year 1754 when he was appointed ambassador to Rome, his first important civilian post. Prior to 1758 he was known as the Count de Stainville, son of François-Joseph de Choiseul, Marquis de Stainville. The latter was a diplomat in the service of Duke Francis of Lorraine, who became Grand Duke of Tuscany and eventually Emperor Francis I. As the subtitle indicates, Butler's book is a biography of both the elder and the younger Stainville.

This volume of 1,078 pages of text is divided into four parts. Part 1, entitled "Beginning, 1719–1740," describes the origins of the family in Lorraine and the diplomatic missions of the Marquis de Stainville. The younger Stainville was commissioned in the French army at the age of eleven and fought for France throughout the War of Austrian Succession. That father and son served rival masters was not so paradoxical in the eighteenth century as it would be at other times. In fact,

the careers of both contributed to the same end, the eventual rapprochement between the Habsburgs and the Bourbons. Part 2, "Campaigning, 1740–1743," finds the young Stainville taking part in some of the most famous campaigns of the war, including the occupation of Prague and the perilous retreat of the French army in the winter of 1742. Part 3, "Commanding, 1744–1748," describes the brilliant career of the younger Stainville as regimental and brigade commander and his final promotion to the rank of major general at the age of 28. Finally, part 4, "Climbing, 1748–1754," shows the Count de Stainville struggling to the top in the court politics of Versailles, forming strong friendships, and making use of powerful protectors, among them the marquise de Pompadour.

A vivid personality emerges from these pages. Stainville's extraordinary intelligence combined with a gaiety of temperament more than offset a rather homely physiognomy. He was successful in the salon and with women. But his wit could be mordant and stinging. He was thought by contemporaries to be the model for Jean-Baptiste Gresset's chief character in his comedy, *Le Méchant*. But Stainville was also a *philosophe sans le savoir*, an unpedantic philosopher who corresponded with Voltaire and was deeply influenced by Montesquieu. He had more than the dilettante's knowledge of painting, music, literature, and drama. "He was," writes Butler, "one of the most artistically implicated men to hold high political office in any age" (p. 799).

Some may question whether 1,078 pages are necessary to depict the early career of "the future Choiseul." But this book is much more than a biography: it is a portrait of the age itself. The general reader, the student, and the specialist alike will find this an illuminating treatment of that most complex of periods, the second quarter of the eighteenth century. The most important original contribution is Butler's detailed description of the origins of the diplomatic revolution and the hitherto unknown part played in it by the elder Stainville. And Butler moves easily into social history, describing the management of the Stainville estates in Lorraine and the transition from late feudalism to "middle-class feudalism" and the coming of industrialism. Cultural, intellectual, and political topics are treated with competence. The style is agreeable, and at the end the reader looks forward to the appearance of the next volume.

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HAYDEN MASON. *Voltaire: A Biography*. Baltimore: Johns Hopkins University Press. 1981. Pp. xiii, 194. \$14.95.

There should be some compelling reason to publish a short biography of Voltaire at this point in Enlightenment studies. Unfortunately I can find none in the present work.

If the student or specialist needed information regarding an obscure detail of Voltaire's life, the enormous and fascinating correspondence, now available in a modern edition edited by the late Theodore Besterman, would be the obvious place to look. Voltaire tells his own life story in minute detail. To correct his exaggerations, megalomania, and self-vindication, the vast monographic literature spawned by the Voltaire academic industry could be consulted. *Studies on Voltaire and the Eighteenth Century*, also largely an inspiration and investment of Besterman, now contains hundreds of volumes, and no end is in sight, nor should there be. Voltaire is the model philosophe. The more we learn about him—his quarrels, his opinions, his literary inventions—the more we learn about the Enlightenment, at least the high Enlightenment of the philosophes and the *salonnières*. If the general reader wishes a short, spirited assessment, Gustave Lanson's *Voltaire* (1910) is still refreshing. *The Intellectual Development of Voltaire* (1969) by Ira Wade would be the logical place to go for an update on the status of scholarly views. For a political slant as well as a continuation of the Lanson vision of Voltaire as a responsible political animal, Peter Gay's *Voltaire's Politics* (1965) remains engaging and provocative.

It is not the modesty of Mason's book that is troublesome. Lanson is no longer. Mason is vapid. The work has no thesis that one could dispute or affirm, and more importantly for a biography, it contains no sense of intimacy that might bring the reader closer to the awesome figure of Voltaire. We follow Voltaire from youth to old age and death: from France to England, to Prussia, to Ferney, along well-traveled roads. We may glimpse his friends, his lover, his patrons, but we get close to no one. Too much attention is paid to Voltaire's hypochondria, which is one of the more tiresome threads running through the letters, and too many fatuous comments by contemporaries and later admirers are given special attention. Is there any reason to quote Goldsmith's comment that no man "can truly be said to have lived" more than Voltaire, or to remember that Flaubert called him a "Saint"? Voltaire would have cringed at that no matter what the context.

We ask a lot of biography these days, especially biographies of eighteenth-century types. Johnson alerted the world to the pitfalls of this art and laid down some stringent requirements. There will always be room for the short account that invites the reader to delve more deeply or offers an original flash, but this is not one of them. What would serve Voltaire best would be a study similar to the one Arthur Wilson made of *Diderot* (1972), and Bester-

man approached Wilson's achievement in his bulky but uncritical *Voltaire* (1969). Wilson's was the work of a lifetime and the product of extraordinary sensibility, a model of historical biography. It took Wilson thirty years to become tolerant as well as knowledgeable about the myriad of controversies that attended the life of a philosophe. The most valuable aspect of this study, apart from its easy readability, is the chronology appended to the end, where events in Voltaire's life are set against other notable contemporary happenings.

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CHARLES COULSTON GILLISPIE. *Science and Polity in France at the End of the Old Regime*. Princeton: Princeton University Press. 1980. Pp. xii, 601. \$40.00.

The editor of the *Dictionary of Scientific Biography* enjoys the company of craftsmen, experts, scientists, and public servants, particularly if they lived in France on the eve of the Revolution. In this treasure chest of a book, he explores how several hundred of these men made science serve the "polity" and vice versa. The book has no hero but science; in another age, the many busy actors might have been the nameless artificers who built the medieval cathedrals.

But Charles Coulston Gillispie knows his men well (there are no women). He appreciates their work as a fellow scientist, and, as a historian, he interweaves their biographies in order to elucidate their roles. He is both thorough and broad-minded; one is initially surprised to find one-half of the section of "professions" devoted to "charlatans," twenty-seven pages on Mesmer, forty pages on Marat. But then, out of an overlong exploration of "animal magnetism," as distinct from "ferro-magnetism," there emerges an important point: in the hands of the marquis de Puységur hypnosis became an effective therapeutic technique. As for Marat, Gillispie notes that none of his biographers seems to have read his scientific books. He proceeds to discuss these as one who really understands the materials, apparatus, and techniques employed, which enables Gillispie to destroy any claim to originality made by the revolutionary firebrand.

In an attempt to avoid monotony of approach in a work with so many actors, Gillispie opts for the hallowed Gallic division into three parts: "Institutions," "Professions," "Applications," thus attempting to provide some dramatic tension.

Among the men whom Gillispie admires, Turgot ranks first, because of his many creative initiatives. From him, the main threads lead to Vicq d'Azyr in medicine and public health, to Condorcet in mathe-

matics and its applications, to Lavoisier in laboratory science and modern chemistry.

Gillispie's method is leisurely scholarship—a favorite word is “browsing”—which is no doubt why the book has been so long in the making. He applies that method, for example, to the two-hundred-odd uninventoried boxes in the archives of the National Academy of Medicine, not, like the *Annalistes*, in search of materials for the computer, but rather in quest of scholarly impressions. These result in a picture of what the Royal Society of Medicine must have been: a group of hard-working medical scientists tied to a national network, determined to raise health standards, improve disease prevention and sanitation, speed royal help in emergencies, and encourage local doctor-scientists in serving the “polity.” Gillispie records many case histories for “the vicarious experience they give of the actual meaning of enlightenment.” The doctors “make a favorable impression on the whole . . . the record gives no sense that they predicated their services upon ability to pay.” *Pace*, Michel Foucault!

Gillispie's portraits range from famous to minor actors—one of these, Duhamel du Monceau, expert at rope-making, silviculture, ship-rigging, and agronomy, published a treatise on stained glass windows “by the last surviving master of a craft,” and another, “the first comprehensive work” on pipe organs. One leaves this book with the impression of late eighteenth-century France as an intensely busy place, full of stimulating people. The fine translations make good reading, and the conclusion makes us think. We are promised a second volume.

Denis Diderot would have liked this book.

DORA B. WEINER
Manhattanville College

WILLIAM DOYLE. *Origins of the French Revolution*. New York: Oxford University Press. 1980. Pp. 247. Cloth \$37.50, paper \$14.95.

This book is intended to move the debate about the origins and meaning of the French Revolution beyond “old orthodoxies . . . now in urgent need of burial” (p. 3). William Doyle believes that the consensus associated with the names Albert Mathiez, Ernest Labrousse, and Georges Lefebvre has collapsed “under the onslaught of new research” (p. 1), but he also thinks that “those who have so revelled in the joy of destruction” should “declare what they now think the general picture to be” (p. 2). Doyle is the author of *The Parlement of Bordeaux and the End of the Old Regime 1771–1790* (1974) and *The Old European Order 1660–1800* (1978), as well as of scholarly articles on the parlements and the aristocracy. The present work invites comparison with Georges Lefebvre's *Quatre-vingt-neuf* (1939), translated by Rob-

ert R. Palmer as *The Coming of the French Revolution* (1947), for both combine topical chapters with narratives and suggest interpretations of the whole revolution. Doyle's part 1 assesses much of the literature since 1939; his part 2 offers his own views. The footnotes serve as a bibliography, aided by a useful index of authors cited.

Doyle's book will be of interest for his able summaries of the best available information, skillfully woven into a narrative, and for his stand on the Cobban-Lefebvre controversy. His introductory essay on this subject is a convenient survey with a pronounced emphasis on revisions of Lefebvre's interpretations. In his part 2 Doyle opens with what is essentially a political history of the monarchy's downfall “because of its own inner contradictions” (p. 115). There was no social crisis, no class struggle; feudalism was a reality but its benefits were shared by nobles and bourgeois, as were those of early capitalism. Opinion was becoming a force, nurtured less by the philosophes than by disgust at the court's blunders. Yet in late 1788 and 1789 social and economic forces erupted into events and changed their course, which Doyle still interprets as primarily a political revolution, although one with momentous social side effects. His central theme is the shift to a representative system acceptable to a propertied elite of nobles and bourgeois, the sort of “notables” who were to persist through the nineteenth-century regimes. Doyle recognizes newly awakened class hostilities in the conjuncture of economic hardships and political tensions that, via popular uprisings, cut the knot preventing equal citizenship within the notables and pressured them into abolishing feudalism. His story of origins ends with the codification of these changes by the August 11 decrees and the Declaration of the Rights of Man.

Doyle's book is important and not to be overlooked, but in the opinion of this reviewer he has achieved precision at the cost of limiting the use of his evidence and narrowing the focus of his interpretation. His treatment of the “old orthodoxies” does not do justice to Lefebvre, whose best thought was not so much dependent on his Marxist terminology, which appears crude when removed from its contexts (see Doyle's sketch on pp. 7–9), as on his appreciation of the interaction of political with social life at all levels, including economic activities and popular mentalities. This the Lefebvre classic superbly exhibits, and most of the studies Doyle cites could find a place in Lefebvre's interpretation. Doyle's accumulated evidence qualifies but does not refute what Mathiez called “the disequilibrium of the classes.” Doyle, like Lefebvre, finds a bourgeois revolution in 1789, and both understand it to comprise the integration of the bourgeoisie into French political life, along with other social reforms. Doyle's notables are no different from Lefebvre's. What one

misses in Doyle's book is certainly not information but rather its use in explaining the long-term background of the political breakdown and the succession struggle—the long preparation for 1789 and for the duration and seriousness of the revolutionary process after the first grip of the notables loosened.

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ANTOINETTE WILLS. *Crime and Punishment in Revolutionary Paris*. (Contributions in Legal Studies, number 15.) Westport, Conn.: Greenwood Press. 1981. Pp. xxi, 227. \$32.50.

Destroyed by the flames that engulfed a number of public buildings in Paris during the last days of the Commune in 1871 were documents of great potential interest to the historian, including most of the records of Parisian criminal courts during the early Revolution. The series Z³, containing the records of six temporary courts that took over the caseload of the Châtelet in 1791 and 1792, alone survived. Through her research at the Archives Nationales in this long-ignored and misfiled series of documents, Antoinette Wills has begun to fill in the gap in our knowledge of crime and justice in early revolutionary Paris.

Wills divides her monograph into two parts. In the first she examines eighteenth-century criticism of Old Regime justice, the criminal legislation of the early Revolution, and the operation of the criminal justice system created by the Revolution. Through analysis of the punishments imposed by these temporary courts she charts the emergence of the prison sentence as a penalty ostensibly aimed at rehabilitation of the criminal.

In the second part of her study Wills examines the nature of Parisian crime. Her findings confirm that the early Revolution brought little change to the pattern of crime familiar to students of Old Regime Paris. Theft continued to be the most frequently reported crime, followed distantly by fraud, assault, and homicide. Among those indicted we find, as in prerevolutionary Paris, a preponderance of young males—often recent arrivals in the city—who were employed in semiskilled and unskilled trades. Wills draws on the wealth of information in French criminal court testimony to illustrate the lifestyle of these working poor. She finds that the Revolution brought an increase in Parisian crime but this conclusion must be accepted with reservation. Wills relies for her Old Regime data on the study of Châtelet cases published in 1971 by a group of scholars under the pseudonym Porhyre Petrovitch (in Abbiatucci *et al.*, *Crimes et criminalité en France:*

17^e–18^e siècles). This study has been criticized by Alfred Soman ("Deviance and Criminal Justice in Western Europe, 1300–1800: An Essay in Structure," *Criminal Justice History*, [1980]: 8) for its exclusion of incomplete cases filed among the papers of the commissaires of the Châtelet. Moreover, Wills accounts for this crime increase by asserting that the Revolution forced many to crime simply to survive, but she does not present evidence, such as bread prices, to substantiate her claim.

Despite these reservations, however, Wills's book is a contribution to our knowledge of how Old Regime criminal justice was replaced by a new order. Wills even includes appendixes summarizing the differences between Old Regime and Revolutionary jurisprudence.

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GEOFFREY ELLIS. *Napoleon's Continental Blockade: The Case of Alsace*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1981. Pp. xvi, 355. \$49.95.

Previous works in English on Napoleon's Continental System have largely studied its impact on coastal areas. Geoffrey Ellis's book emphasizes the commerce and industry of Alsace (the departments of Haut- and Bas-Rhin) from 1803 to 1813. As an industrial area and international crossroads, Alsace was situated to benefit from unique economic opportunities. But it also reflected the general economic problems of the empire. Thus Ellis's work illuminates far more than just the economy of one province. It does not, however, deal extensively with agriculture, which, as Ellis recognizes, accounted for over three-fourths of the empire's gross national product.

Ellis draws a clear distinction between Napoleon's "Continental System" and the "Continental Blockade." The former he defines as the entire scope of Napoleon's European policies, whereas the latter refers to the economic policies involved in the Berlin and Milan decrees. These, he says, were intended to give France economic hegemony over Europe to match its military and political dominance and to give it markets in Europe that British naval superiority denied it overseas. Therein, according to Ellis, lay a major weakness of what he calls the "French continental market design," for the nations of Europe were not willing to sacrifice their own interests to enable the French to replace British economic supremacy.

The blockade's impact on Alsace varied considerably. But Ellis shows that in general the period from 1807 to 1810 brought expansion to trade, industry, and agriculture in Alsace as well as throughout

much of inland France. Strasbourg, the commercial center of Alsace and hub of international trade routes, especially benefited because the blockade shifted trade inland away from coastal routes and opened alternative markets, primarily in Germany. The industry of Haut-Rhin likewise expanded rapidly during this period, although much prosperity came not from exports but from increased domestic demand, which actually was the most important factor in Alsatian economic growth. The economic crisis of 1810 hurt Alsace, especially the tobacco monopoly and the shifting of Levantine cotton imports from Rhenish cities to Italy and the Illyrian provinces. But Ellis blames much of this slump on causes other than the blockade. Indeed, rarely during this entire period were conditions as bad as Strasbourg's chamber of commerce alleged, and many actual difficulties antedated the blockade and even the Revolution. Thus, Ellis concludes, the blockade brought more positive results than historians have previously recognized, but by no means did it directly cause all economic changes.

Ellis's work is thoroughly documented from extensive archival research and statistical analysis. Regrettably the available data are concentrated on the industrial and commercial magnates, especially those of Strasbourg, instead of being spread more evenly across the economic spectrum. Some lengthy quotations in French might have been translated, and the six pages explaining terminology should have come early in the book, not halfway through it. These points aside, this book is a valuable contribution to the economic history of Europe, the empire, and Alsace in particular. It is a "must" for any serious student of the period.

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JAMES R. LEHNING. *The Peasants of Marthes: Economic Development and Family Organization in Nineteenth-Century France*. Chapel Hill: University of North Carolina Press. 1980. Pp. xiv, 218. \$19.50.

It is now more than a century since Marx insisted that the expansion of capitalism penetrated the entire social fabric—rural life as well as urban. Raymond Williams and Eric Hobsbawm, among others, have insisted on the turbulent transformation of the English countryside, and Charles Tilly and his students have been applying the lesson to French social history. The penetration of capitalism and the state into French rural life constituted a long and complex process, replete with regional variation. But it was well under way by 1750 and left no enclave of traditional, custom-bound peasant life untouched. In *The Peasants of Marthes*, James R. Lehning analyzes this process, which he chooses to

call rural urbanization, in a single French village located in the Pilat mountains, south of the industrial city of St. Étienne, between the July Monarchy and World War I.

Lehning scrupulously traces the relation between economic development and family organization in Marthes, emphasizing the changes and increase in market activities that he links to changing patterns of rural production in general and women's work in particular. Moving from the growth of the urban economy and the rural community's experience of economic transformation to demographic problems, Lehning explores seasonality, mortality, nuptiality, fertility, and migration. In his final chapters, he considers the structure of the peasant family, the relationship of the family to property and inheritance, and three types of family economy.

The core of the work lies in Lehning's sensitive demographic analysis, on the basis of which he concludes that throughout the period he is studying the peasant family remained nuclear, albeit occasionally extended or augmented by near kin. In Lehning's view, the peasant family responded to economic change with strategies of adaptation intended, above all, to preserve "the family homestead as a basis for a limited kinship group" and to support "the family group through the contributions of all its able-bodied members" (p. 9). These goals of the peasant family persisted throughout the period, encompassing rather than being determined by the eddies of economic change.

Lehning bases his conclusions on demographic and related statistical evidence. Aside from a few illustrative family reconstitutions, his study rests on ecological analysis and more cross-sectional than time-series data. Within these contours, his work is exemplary and should prove invaluable to scholars not merely in French social history, but in related subjects as well. The very characteristics that contribute to the parsimony and internal coherence of his analysis nonetheless leave the reader impatient for more. One misses the contribution of the ethnographer, the cultural historian, even the antiquarian. The events that interest Lehning are demographic—births, marriages, and deaths. But what of the historically specific and subjective experience of those events? We do not get so much as a verbal picture of the homestead that he claims "symbolized" family life. He notes the intense Catholicism of the region but gives us no chapels, no priests, no communions. It is proverbially difficult to penetrate the internal life of French villages, not to mention families, but he does not attempt to trace the human manifestations of the processes that do effect such penetration. What he does do is genuinely important, indeed indispensable, and he does it impressively. But the lives that experienced such change were fraught with joys, sorrows, and conflicts—

between classes, genders, and generations—that have their own social meaning.

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MARC TRACHTENBERG. *Reparation in World Politics: France and European Economic Diplomacy, 1916–1923*. New York: Columbia University Press. 1980. Pp. x, 423. \$25.00.

Emblematic of an allegedly Carthaginian peace, reparations have long been identified as a primary weapon in France's diplomatic arsenal, utilized by successive governments with deliberate intent to humble Germany and to lay waste the foundations of its power. Observers and historians alike have exhibited little discomfort with the widely shared assumptions that French policy toward Germany in the years from Versailles to the occupation of the Ruhr was cut from whole cloth, that it had an intelligible unity, and that it was pursued relentlessly. Like much else in interwar France, reparations policy may have followed a course marked by inelegant zigzags: where plans seemed sure there were perhaps no plans at all. Above all, things were not what they seemed to be.

It is the striking merit of Marc Trachtenberg's study to point out what should have been obvious but has been strangely elusive in this instance: that politicians do not always mean what they say or say what they really want. There is no compelling reason to take the French government at its word, for it did pursue a policy often at odds with its declarations. Prudence, caution, and realism were also in the French arsenal, and moderation was not repellent either to the tiger Clemenceau or, up to a point, to the fighter Poincaré. Truculence and threat should not be mistaken for policy, Trachtenberg reminds us, especially when the policy itself was daring in its moderation. Skillfully and meticulously, the argument is put that the French, despite their oscillations, sought not to ruin Germany, but to repair it. At Versailles the French employed maximum demands in a fruitless effort to win aid and guarantees from their allies; afterward they sought alternative solutions, all of which looked toward economic cooperation with Germany. The real problem, Trachtenberg believes, was not Germany's capacity to pay but the Allies' reluctance to take its goods. Even when Poincaré marched on the Ruhr he had no clear idea of what he wanted to accomplish, but surely he did not aim to cripple Germany.

Despite its title, this book has little to do with "world politics." Rather it is about economic policy, reparations, and French politics and attitudes toward the power—real and potential—across the Rhine. Attention is devoted to French ideas, inten-

tions, and actions. Within these boundaries it succeeds in provoking new understanding of and arguments about a very old question.

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ANGEL VIÑAS *et al.* *Política comercial exterior en España, 1931–1975*. In two volumes. Madrid: Servicio de Estudios Económicos, Banco Exterior de España. 1979. Pp. xxviii, 740; xvii, 741–1,564.

This work has been published to celebrate the fiftieth anniversary of the Banco Exterior de España, founded in 1929. The purpose of Angel Viñas *et al.* is to analyze the foreign-trade policies of the Franco dictatorship. The authors also provide an introduction about the Second Republic.

The book clearly belongs with the growing body of serious monographs that are being published in Spain with the aim of submitting the policies of the Franco regime to careful scrutiny. This is a task that could not be undertaken while the dictator was in power not only because of the strict censorship that, though somewhat mitigated in the final years, was one of the mainstays of the regime but also because of the secretive and suspicious nature of the dictator and his governments, which made access to the main sources impossible. With the restoration of freedom, most sources (though by no means all) have become accessible, and books such as this one are now possible.

Viñas, head of the team of authors, is one of the most prolific users of the newly available material and is a specialist in diplomatic-economic history of the Franco era (including the Civil War). His archival reach is impressive and his analyses are often penetrating; he seems, however, to be unable to select and summarize his material. Most of the books he has written could have been reduced to article length. This is again the case with the work under review: it would have immensely benefited had a good editor reduced its size by two-thirds or more. The whole book is burdened with superfluous and unnecessarily long quotations, annoying repetitions, unessential detail, and hackneyed formulas (impossible to render into English).

All of this does not detract from solid merits. First, the topic is crucial to an understanding of Francoism; foreign-trade policy was one of the most characteristic areas of the regime's economics. It reflected its failures and successes in a remarkably transparent way: foreign trade and foreign relations in general were a strong spur for the evolution of the dictatorship. From its first moments in 1939 the regime made a virtue of necessity: it had inherited a cumbersome mechanism of foreign-trade intervention from a decade of depression and civil war. Imbued with fascist ideology, the government not

only retained but also reinforced this mechanism in the name of "autarky," heaping scorn upon liberalism and free trade. The fascinating fact for the historian is that it could cling to that clumsy tangle of multiple exchange rates, import and export licenses, special operations, bilateral agreements, combined accounts, prohibitions, quotas, and whatnot for twenty long years. When, forced by the long-impending suspension of external payments, the dictatorship finally accepted a certain measure of liberalization, it did so reluctantly and half-heartedly, as though forced by the circumstances to betray its lifelong ideals. Paradoxically, this betrayal produced fifteen years of economic growth and the regime's most solid claim to a legitimacy of sorts. To the very end, however, the regime and the *caudillo* were mistrustful of free trade and *laissez faire*, which in fact they never put into practice. During the 1960s and early 1970s liberalization meant only a softening of the former iron-fisted economic intervention: multiple exchange rates were replaced by a single realistic rate, quotas and prohibitions were partially replaced by a highly protectionist tariff, the movement of capital and the transfer of technology were made less difficult, and the exportation of labor became almost totally free.

To the authors' credit, the cause-effect relationship between the incompetence (and venality) of the policy makers and the low standard of living of the Spanish people is clearly shown. One misses, however, a more thorough analysis of trade in itself and its relation with the rest of the economy for the period of autarky. The final chapter carries out this analysis for the 1960–75 period, and it is, though not the most original, the most satisfactory chapter of the whole book. For the American reader, the parts dealing with the origins and development of the Franco-U.S. pacts of 1953 (chaps. 5, 6, and part of 4), which show how essential they were to the regime's survival, may be of special interest.

Another merit of the book is the admirable array of sources. For the first time the papers of the Instituto Español de Moneda Extranjera (IEME, one of the key links in the controlling mesh) have been painstakingly studied, together with a variety of Spanish, German, and U.S. archives. From now on, no historian of the period can afford to ignore this book; it will provide material for diplomatic and economic historians for years to come. Unfortunately, it cannot be the definitive work on the topic. It is too raw, too unfinished. The reader often gets lost in it, and, after more than 1,500 pages, there are no conclusions, not even a summary. Other works will be needed to re-order and interpret the trove of new material that Viñas and his collaborators have brought to light.

GABRIEL TORTELLA
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SALLY MARKS. *Innocent Abroad: Belgium at the Paris Peace Conference of 1919*. Chapel Hill: University of North Carolina Press. 1981. Pp. xviii, 461. \$26.00.

In *Innocent Abroad* Sally Marks examines Belgian foreign policy objectives and diplomatic tactics at the Paris Peace Conference. She emphasizes that Belgium achieved very few of its goals, and she carefully analyzes the reasons for Belgium's diplomatic failures. In the course of this examination, Marks elucidates aspects of British, French, and Dutch diplomacy as well as that of Belgium and Luxembourg. Her research is very thorough. She has made excellent use of archival materials and manuscript collections in Belgium, Britain, and France and has supplemented these sources with printed documents from Holland and Luxembourg as well as with German and American published and unpublished materials.

In chapters that are arranged topically Marks illuminates Belgian diplomatic goals. Belgian officials sought to revise the 1839 treaties, abolish compulsory neutrality, achieve greater independence of action, and secure guarantees against future German aggression. Likewise, Belgian leaders wanted to annex small slices of land from Holland and Germany, secure full reparations for both damage inflicted and war costs, gain control of Luxembourg, and broaden the Belgian Congo's Atlantic coastline. This constituted a long list of objectives, and Marks demonstrates that Belgian leaders had neither the ability nor power to accomplish most of these goals. She emphasizes the inexperience and naiveté of Belgian policy makers. They foolishly believed the rhetoric of the great powers and mistakenly overestimated not only British and French good will but also their resources. Marks incisively examines how Belgian leaders were unable to recognize the conflicting nature of Belgian foreign policy goals, to sort out priorities, and to resolve the tension between their pursuit of full national independence and unrestricted freedom of diplomatic action on the one hand and their quest for assured security on the other hand. But even had Belgian officials shown more astuteness, they would not have had the bargaining power and diplomatic leverage to overcome Britain's intent to distance itself militarily from the Continent, France's desire to incorporate Belgium and Luxembourg within the French orbit, Holland's fear of Germany and preference for neutrality, and all the European Allies' desire for larger reparations.

Marks does a particularly fine job delineating the conflicts within the victorious coalition. She stresses Belgian antipathy to becoming a French satellite, British indifference to Belgian security imperatives and fear of Belgian economic competition, and French determination to get their own way in

Luxembourg. At the peace conference the Dutch shrewdly capitalized upon these divisions and gained British support to withstand Belgian claims. By the mid-1920s, however, Marks contends that Belgian leaders had gained considerable experience. They then endeavored to reconcile Franco-British differences and thereby preserve an Allied balance against Germany.

This is conventional diplomatic history. The emphasis is on the interaction of governments and the interplay of national leaders. By focusing much attention on diplomatic exchanges, conference proceedings, and personal accounts, Marks has recreated the texture of diplomacy at the Paris Peace Conference, at least as experienced and perceived by Belgian policy makers. She carefully details the obstacles encountered by Belgian officials as they sought to make themselves heard by the Big Three at the conference. She emphasizes the role of Paul Hymans, the Belgian foreign minister, whose relentless and honorable pursuit of Belgian interests alienated Clemenceau, outraged Lloyd George, and occasionally exasperated Wilson. She illuminates the problems within the British delegation and criticizes the British prime minister for his insensitivity to Belgium's needs and his limited grasp of Britain's long-term interests on the Continent. In contrast, Marks praises the sagacity and finesse of Dutch diplomats.

Because so little has been written on Belgian foreign policy during this period, this volume is a welcome addition to the literature on the Paris Peace Conference of 1919. But the book might have been even better if it had been more imaginatively conceived, carefully organized, and cogently written. The major question that the volume addresses, why Belgium gained so little at the conference, invites a rather simple and straightforward answer; because Belgium had so little power. If Belgian diplomats were not as skillful as their contemporaries, that is of interest, but it hardly constitutes a major contribution to our understanding of international politics and economics. The issue of the volume's significance is compounded by some organizational difficulties. The chapter on "Secondary Skirmishes" seems to be a compendium of either unimportant issues that add little to the major themes of the book or material that might have been integrated more effectively into other chapters. The "Postlude" of over sixty pages vacillates between being a conclusion to this book and an introduction to another one on the 1920s. As a result, the full implications of this narrative for overall European diplomacy in the interwar era do not emerge as clearly as do the details of the diplomacy of Belgium and the other powers at the peace conference itself.

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JONATHAN E. HELMREICH. *Belgium and Europe: A Study in Small Power Diplomacy*. (Issues in Contemporary Politics, Historical and Theoretical Perspectives, number 3.) The Hague: Mouton; distributed by Walter de Gruyter, New York. 1976. Pp. xiii, 451. \$40.00.

The diplomacy of small states forced to operate within a system dominated by large states is of both historical interest and growing importance. Indeed, a special urgency now attaches to the topic with a wide variety of new "emerging" states and a situation where former "great powers" such as Britain and France find themselves in a world dominated by "superpowers." Thus a study of the course of Belgium's diplomatic relations with the major European powers assumes a special relevance. The topic is important, sources for its study are available, and Jonathan E. Helmreich has risen to the occasion in this well-written volume under review.

Belgian diplomacy has been dominated by an overwhelming recognition of its small size. Belgium's location, which makes it a convenient passageway and a potential battlefield, has also markedly influenced its policy makers. A third major consideration has revolved about the concept of neutrality that Belgian leaders zealously tried to follow for well over a century. Helmreich effectively shows how these factors have molded the small nation's policies since 1839. Prior to that year Belgian policy was truculent, as becomes a new revolutionary state. The diplomacy of revolution and independence is ably presented and is perhaps the best brief (34*pp.) presentation one can find in any language. In this century an additional factor has been the Walloon-Fleming linguistic problem, which had direct influence on foreign policy. All these themes are carefully woven into the narrative, which emphasizes the European context in which Belgian leaders conducted affairs.

The author has been deliberately selective in the topics covered and also in the amount of emphasis given those topics. He devotes substantial consideration to security and reparations negotiations after World War I; but colonial attempts prior to the Congo, Crimean War diplomacy, relations with the Vatican, and other topics are omitted as not central to Belgian diplomacy. Helmreich argues that their inclusion would only serve the specialist and "such a tedious process would merely cloud the outlines of the picture . . ." (p. 2). It is certainly correct that their omission makes the main lines of policy unmistakably clear. Such a decision, however, has its price. The author attributes almost exclusive interest in colonization to the crown rather than the government; but under Leopold I, initiatives were shared. Crimean War diplomacy shows Belgium resisting both French and British efforts to compromise its

neutrality, a topic that would have fit well into the author's general presentation. Also relevant were the touchy implications for neutrality surrounding Belgian "volunteers" who went to Mexico during Maximilian's brief reign. At Constantinople prior to the Crimean War, Belgium and the interests of all the great powers were well served by Edouard Blondeel van Cuelebroeck. An exception to the practice of keeping able diplomats for unusually extended periods at one post, Blondeel effectively served in many crucial locations at critical times but is not mentioned in this book. Neither does Henri de Brouckère's name appear. These are surprising omissions but are explainable by the author's overall rationale and perspective on his subject.

Despite the reservations mentioned above, the volume is impressive. The author has succeeded in his objective and given us a fine presentation of the main themes of Belgian foreign policy since 1830. Instructive and yet disappointing for small nations hoping to derive advantages by having gifted and eloquent spokesmen is the observation by Lloyd George commenting on Paul Hymans: "When he dominated the conversation, he thought he dominated the situation" (p. 221). Therein can be seen the reality and frustrations inherent in small-power diplomacy.

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AKSEL E. CHRISTENSEN. *Kalmarunionen og nordisk politik, 1319–1439* [The Union of Kalmar and Scandinavian Politics, 1319–1439]. Copenhagen: Gyldendal. 1980. Pp. 324. 225 KR.

The distinguished Danish historian Aksel E. Christensen laid the groundwork for this book over a period of almost thirty years. The result is a thoughtful and important study of a period too little known outside Scandinavia. It is a pity that there is no résumé in a non-Scandinavian language, and the book deserves to find an American or British translator.

The survey begins with the regal union of Norway and Sweden in 1319, the result, as Christensen argues, of dynastic chance and not of any pro-union ideology. The author sees the fourteenth century as establishing certain preconditions for Scandinavian political cooperation, notably in the struggles against the dukes of Mecklenburg and the Hanseatic towns.

But it was Margrethe, daughter of Valdemar IV of Denmark, who first framed and in any sense carried through a conscious policy of Scandinavian union. In 1387 her son Oluf, who had ruled all three Scandinavian states only since the previous year, died. Margrethe was able, in the next few years, to bring all three states under the rule of her great-nephew Erik of Pomerania. By this decisive

moment, Christensen believes, feeling in all three countries was in favor of joint government action at home and abroad. In preserving the position of her dynasty, Margrethe was also capitalizing on this feeling.

In 1397 Erik was crowned union king at Kalmar. From discussions at Kalmar there emerged a document attesting the coronation and a "union letter" of uncertain status. Of the discussions themselves we know nothing. Christensen provides a full account of the historiography of this episode, followed by his own close analysis of the two documents, taken singly and in comparison with one another.

His conclusion is that at Kalmar Margrethe achieved a personal political triumph by avoiding a clash with the aristocracy of the three kingdoms and securing Erik's position. The controversial "union letter" did not in itself constitute an unambiguous legal basis for union. Kalmar gave Margrethe the opportunity to undertake future negotiations in each kingdom toward a fully articulated union constitution; it also gave her the opportunity to set further constitution making aside and get on with the business of ruling. As a practical politician she chose the latter course.

After her death, however, Erik showed that he lacked her shrewd pragmatic touch, and in 1439 his Danish and Swedish councils declared him deposed. The power of aristocracy thus triumphed over the idea of a strong union monarchy, and it remained to be seen whether a community of feeling among aristocrats in the different countries (*adelsskandinavismen*) could provide an alternative focus for a lasting union.

Earlier historians, with the natural human desire to see a pattern in human affairs, have sometimes been drawn to see the Kalmar Union as something more planned and long term than merely the result of dynastic accident and the opportunism of Margrethe. Christensen does not discount the place of ideas in politics, but he also emphasizes the complexities and the short-term crosscurrents of the situation. All one can add about a book much too rich to summarize easily is that his account carries conviction. Its value is enhanced by some pleasant facsimiles of seals and documents.

JOHN SIMPSON
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H. G. HAILE. *Luther: An Experiment in Biography*. Garden City, N.Y.: Doubleday. 1980. Pp. x, 422. \$14.95.

JEAN WIRTH. *Luther: Étude d'histoire religieuse*. (Travaux d'Histoire Éthico-Politique, number 36.) Geneva: Librairie Droz. 1981. Pp. 158.

Old man Luther, the aging Luther, as H. G. Haile observes, has never commanded much interest

among historians and has even been an embarrassment to Lutheran theologians. Yet the aging Luther warrants our careful attention not only as an interesting counterpart to young man Luther but more significantly for the light that he may cast upon himself and his work once he became in his own eyes a historical figure. For one so central to the Reformation and so voluble about himself and his past as was Luther, such a biographical experiment would appear to be both long overdue for understanding the man and fruitful for biography in general.

The visit of the papal nuncio Vergerio in 1535 sets the stage for this study of the aging Luther and formally delimits Luther's last ten years as the temporal focus for the book. We have here the jowly, sardonic reformer recently emerged from the completion of the translated Bible; the consummate actor or calculating Machiavelli of the Wittenberg Concord; the compulsive writer and versatile publicist; the increasingly irritable, tending to savage polemicist; the relentless prosecutor of Duke George, the archbishop of Mainz, and his own former colleague and close friend, John Agricola; and finally the hateful composer of the late antisemitic writings. Beyond the fact that Luther amid his growing afflictions is convincingly treated, the chief value of Haile's biography for some will be the identification of the effects of the stone that beset the reformer at Schmalkalden in 1537 and from which he almost died. The retention of urine creates an uremic poisoning among whose effects are "an undercurrent of irritability" along with "striking swings between states of confusion and mental lucidity." The author does not use the probable presence of such a psychosis in Luther's case to excuse the horror of the antisemitic tracts but rather to make his subject more understandable. To the end Luther remained himself and is represented as one who was able to draw enormous emotional powers from the depths of his unconscious being and to place them at the disposal of a keen intellect.

Although the biography adheres to the formal concentration on the last ten years, the real achievement arises from its skillful recovery of themes and issues originating earlier. Two other reasons, the first psychological, the second hermeneutical, militate toward the extension of the biographical gaze from the vantage of the last years over the entirety of the reformer's life. First, if aging, as the author avers, is to be understood as a person's accumulation of injuries, then Luther aged considerably from the events of 1525. Second, attacking the much-abused practice of trying to reconstruct Luther's childhood and early career from musings that occurred after 1530, Haile correctly warns that they tell us chiefly about Luther's personality at the time of utterance.

In short, for those who continue to delve into the *Tischreden* to reconstruct history and, most elusive of all, Luther's childhood, we need the control of becoming better acquainted beforehand with this old fellow.

At times a certain historical naiveté on the author's part may create misconceptions for his unwary reader: Luther proclaimed "the rise of the bourgeoisie" (p. 88); John Frederick "acceded to the throne in 1532" (p. 96); all Anabaptists, Münsterites included, constituted a single sect "requiring rebaptism" (p. 125); Melancthon sought at Augsburg in 1530 a "centrist position between Rome and the most extreme Protestants" (p. 130). There are also four plain errors: Tyndale is represented as "the gentle scholar" apparently arrested by Henry VIII's agents—in Brussels (p. 207); "the stinging defeat of imperial forces at Mohacz in 1528" (p. 95) should properly refer to the total catastrophe suffered by the Hungarians in 1526; whatever hopes Luther ever entertained for the proposed council of Mantua in 1536–37 are here so exaggerated as to make nonsense out of *sola scriptura* as his ultimate authority (p. 208); and neither the Ernestine electoral nor the imperial resident chancellery could have been at Meissen under Frederick the Wise (p. 338) but at Torgau and Mainz respectively. Yet in a study that concentrates on the man rather than on his works and world, such errors do not materially impair an effective and challenging approach to the understanding of Luther.

Whereas Haile brings to his subject the sensitivity of a Germanist, Jean Wirth comes to the reformer as a scholar of iconography in its application to late medieval religious mentalities. Wirth's Luther shares many features with Haile's. Both works originate from Urbana, and the Frenchman acknowledges the aid of the American scholar. Both seek to "rehistoricize" their subject. Both take seriously the authenticity of the diabolic for Luther. Both perceive his capacity for playacting. Both emphasize the central importance of the law. Both shed additional light on the "*Türmerlebnis*": in contrast to Haile's clarification of the "spoof" concerning the *cloaca* in the tower as simply a contemporary religious metaphor, however, Wirth provides iconographic support for his conviction that the Reformation was born in a heated room favorable, as traditional belief would have it, to diabolic apparitions. Yet whereas Haile can make understandable the contradictions of his subject, Wirth intentionally seeks to break down the resolution of these tensions and contradictions. Luther is portrayed as a man whose reform of a critical and destructive nature was genuine enough before 1521 but whose reaction to the holocaust of the peasants' rebellion caused him to reshape his evangelical faith into a theological

system of constraint. He undertook a re-Catholicization of a diabolical sort in which the law was applied to the ordering of the peasant masses and the Gospel was reserved for a Christian elite. Luther even sought to delude his contemporaries and future investigators by calculatingly refashioning his own image. He became a euhemerist, a skeptic, a saturnist, and a false prophet.

To compress Wirth's complex argument and insights into such limited space is unfair and potentially misleading, for his work is not to be dismissed as simply a sophisticated amalgam of Denifle, Maritain, and Osborne. Nevertheless scholars will object on at least two accounts. First, Wirth attempts to demonstrate that before 1519 the righteousness of God had for Luther merely a negative meaning as a *machine de guerre* against works; only after the break with Rome was it refashioned to have an evangelical ring. Thus Wirth misses the positive import of Luther's theology and ends by asserting that Luther's fundamental inspiration was a struggle against abuses. Second, the author cannot accept Luther's dialectical, existential theology of paradox on its own terms but insists upon treating it in terms of the coherence of a philosophical system. Consequently it is no surprise that Luther emerges as neither a theologian nor a thinker.

By way of appraisal one might introduce the Eriksonian concept, foreign to both scholars, of the crisis of generativity. Disturbing for any aging person, it was bound to be tumultuous for one who had rejected the immemorial authority of the medieval church. Whereas Haile's Luther holds together convincingly—as the historical Luther so narrowly did—Wirth's fails to convince this reader. Ultimately the crucial issue is not psychological, but hermeneutical. It is not that the saturnine, the skeptical, and the euhemeristic are not present along with the evangelical in Luther, but to make his case Wirth, drawing heavily from the *Tischreden*, has pushed to the breaking point some of Luther's occasional statements.

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ROSEMARIE AULINGER. *Das Bild des Reichstages im 16. Jahrhundert: Beiträge zu einer typologischen Analyse schriftlicher und bildlicher Quellen.* (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 18.) Göttingen: Vandenhoeck und Ruprecht. 1980. Pp. 490. DM 140.

Rosemarie Aulinger's book represents a revised version of her 1974–75 University of Vienna disser-

tation examining the German Diet in the sixteenth century. The author currently is engaged in editing volume 10 of the documentary series *Deutsche Reichstagsakten unter Karl V.*, which will provide source materials for the Diet of Regensburg, 1532.

As Aulinger notes in her evaluation of previous literature, there are numerous scholarly studies pertaining to the sixteenth-century Diet. Some focus on individual meetings, others on specific constitutional issues, procedural matters, and the like. The major contribution of the present work consists of a comprehensive portrayal of the Diet and its personnel in their social and cultural as well as political context. To accomplish her purpose, the author has utilized—and commented upon—a wider variety of source materials than are customarily exploited by historians. Especially noteworthy is the pioneering effort to assemble and systematically evaluate the surviving visual evidence. The catalogue of relevant pictorial works at the end of the text lists and describes over two hundred items; the book also contains thirty-six photographic illustrations.

Aulinger's study should prove useful in a number of different ways. For some readers, her detailed description of the structure and workings of the sixteenth-century Diet will provide a welcome orientation. Others will especially appreciate what appears to be a more original contribution of the book, that is, its lengthy analysis of the elaborate ceremonial and varied social festivities associated with meetings of the estates.

The formalistic ritual that was so prominent in all the Diet's proceedings rested upon a traditionalism that became more and more pronounced as the century wore on. Rigidity and resistance to change seem increasingly to have distinguished the German assembly from comparable bodies elsewhere in Europe. According to Aulinger, even the Reformation had less of an impact on the institution than one might have expected. Only a few procedural changes resulted directly from the religious upheaval. One of the most important of these was the refusal of the evangelical party, after their famous Protest at the 1529 Diet of Speyer, to accept the *de facto* principle of majority rule when it impinged upon matters of faith or conscience.

In view of the space allotted in Aulinger's book to consideration of the visual sources, it comes as a disappointment to find that these materials make only a modest contribution to our knowledge of the Diet. Although certain of the woodcuts provide valuable documentation of ceremonial practices, most of the pictorial compositions included here appear to confirm insights gained from written sources rather than yielding new information. A comparison of Aulinger's work with R. W. Scribner's fruitful essay on German peasants in art

or his forthcoming book on pictorial propaganda of the Reformation would suggest that the potential value of visual source material is significantly greater for some areas of historical inquiry than others.

Nonetheless, taken as a whole, Aulinger's book forms an interesting and valuable addition to the literature on the German Diet.

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MACK WALKER. *Johann Jakob Moser and the Holy Roman Empire of the German Nation*. Chapel Hill: University of North Carolina Press. 1981. Pp. xi, 364. \$26.00.

We need books to cut across the categories and periods of German political history only if they do it in a wise manner by respecting tradition and intelligence in people who lived in their own time and place. The German-speaking world suffers from too much historiography and theory. There is not enough digestible general history, especially when it comes to the eighteenth century. Building upon the tradition of early modern German, small-town *Spiessbürgerlichkeit* and *Lokalborniertheit* (of exclusive, duck-pond and back-yard wallowing) that he uncovered so uncommonly well in *German Home Towns: Community, State, and General Estate, 1648–1871* (1971), Mack Walker now steers into the more positive waters of Central European jurisprudence and *Staatsklugheit* or political science. Johann Jakob Moser (1701–85) was a Swabian Lutheran turned pietist, a fiercely independent bourgeois lawyer who published over four hundred works on the law and politics of the Holy Roman Empire and its component territories. Moser held legal and administrative posts and consultancies in states as substantial as Württemberg and Brandenburg-Prussia on the one hand, and as obscure as Hessen-Homburg and Hessen-Hanau on the other. He gave practical legal advice during the elections of Emperors Charles VII and Francis I. Invariably he fell out with his employers on some matter of principle. Only one job he held for any length, a legal consultancy to the territorial estates' small committee in Württemberg. It led to arbitrary imprisonment at the hands of the despotic duke Karl Eugen during the Seven Years' War and to repudiation by the estates' committee on his release. Moser was consulted by all, but trusted by none. He lived by the pen, mercilessly publishing any politically sensitive papers dealing with the high issues and low motives of German politics from the rape of Silesia in 1740 to the hasty judicial reforms of Emperor Joseph II in the 1770s. His own motive was commendable; it was to keep the politics of the Holy Roman Empire alive.

As a person, Moser seems to have been a ruthlessly self-made talent moving from cheeky *superbia* to pietistic grace in an age when land and money gave power, quantities of which he had, although he had inherited none. Since patronage also deprived him of intellectual freedom, he determined to be no ruler's servant. He made money mainly by publishing his own legal compendiums. Here was no up-market Figaro, like so many of his contemporary lawyers. By 1763, four years of prison on a *lettre de cachet* without trial had only reinforced his pugnacity: he wrote that "as long as God allows this constitution of the world, and there is an emperor, pope, electors, princes, imperial estates, diets, subjects, and so on, so long too must there be someone to tell what their rights and duties are." If Moser was that "someone," then the real problem is that he was no reformer extending the basis of tradition by pushing subjects' rights, although he attacked the innovative natural rights schemes and loose, flapping minds of those cameralists, rationalists, and *Plusmacher* who were increasingly in the service of dynasties and governments. Moser saw no way of extending the archaic and selfish, politically negative powers of territorial estates, their committees, and diets. Government officials were responsible to rulers only, since policy was the prerogative of princes and God-given authorities. The representation of subjects and their positive involvement in government, though touched upon in his *Neues Teutsches Staatsrecht* in the 1770s, he never fully developed, even with regard to Württemberg. Here was no equivalent of Edmund Burke, fighting off the rationalists in the German political arena. Even so, Moser came closer than anyone to upholding the cohesive political practice of the Holy Roman Empire at Regensburg, Wetzlar, and Vienna in the interests of the small states, caught between the twists and turns of Austro-Prussian rivalry and collaboration, and territorial subjects' traditional rights everywhere. That he was a precursor of the old Reich patriotism of his son, Friedrich Karl von Moser, and of Justus Moser and J. S. Pütter is well brought out by Walker's careful juxtaposition of public pronouncement and private Protestantism.

Walker convincingly revises the standard view of Moser as a reformer that is given in Reinhard Rürup, *Johann Jakob Moser: Pietismus und Reform* (1965). Walker bases his work on important unpublished materials, including an early autobiographical sketch that was originally scratched between the lines of publications permitted Moser while he was in prison. Biography and political society truly are conjoined, as Walker produces a work of which Wilhelm Dilthey would surely have been proud. There is now no better starting point for gaining a general appreciation, albeit not without careful

thought, of the pros and cons behind the continued vigor of that curious federal system called the Holy Roman Empire of the German Nation in the mid- and later eighteenth century.

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HEINRICH RODEGRA. *Das Gesundheitswesen der Stadt Hamburg im 19. Jahrhundert, unter Berücksichtigung der Medizinalgesetzgebung (1586—1818—1900)*. (Sudhoffs Archiv, Beihefte, number 21.) Wiesbaden: Franz Steiner Verlag. 1979. Pp. viii, 217. DM 44.

We tend to regard state interference in health care as a modern phenomenon. Heinrich Rodegra's book, however, shows that the government's concern for the health of its citizenry has a long history, one that includes mandating the activities and fees of diverse health care practitioners. Furthermore, physicians and governing authorities have a tradition of uniting to ensure policies that will benefit each group.

The title of Rodegra's work is misleading, since the book actually encompasses much more than a history of public health legislation in Hamburg. In order to place his subject in a larger context, Rodegra includes general histories of health legislation and of the rise of municipal physicians in Germany and Italy since the thirteenth century. He also presents a short history of Hamburg from its founding by Charlemagne through its incorporation in the Bismarckian empire. Then, so that we may better comprehend the development of medical legislation in Hamburg, the author concludes with accounts of comparable activities in two other Hanseatic cities, Bremen and Lübeck.

Rodegra's work focuses on the municipal medical ordinance of 1818, which laid the basis for a comprehensive system of health care in Hamburg. The system ultimately encompassed a city health council; educational programs for physicians, surgeons, and pharmacists; and various hospitals. Hamburg, Rodegra claims, had the distinction of being in the vanguard of these developments in the German Reich. Certainly with regard to the building and extension of hospitals, however, it is hard to imagine Hamburg in the forefront. Hospital construction and expansion were marked features of medical development throughout all of Germany in the nineteenth century.

Although the presentation of the material is somewhat lackluster and prosaic, a few prominent themes emerge. One is that various epidemics throughout the centuries provided the impetus for the introduction, revision, or widening of rules concerning the system of health care. Another

theme is that, at all times, it was the physicians who played the dominant role among the various health practitioners, deciding policy for the apothecaries, midwives, surgeons, and oculists. Perhaps of the greatest significance is the clear evidence that it was precisely the early interest of Hamburg's citizens in their self-government that laid the groundwork for the city's decisive involvement in regulating health care in the nineteenth century.

Rodegra's book is a worthwhile, even if not imaginative, contribution to the history of public health. It contains the full texts of many documents that medical historians may find useful. Of course, such copious, long quotations often make for tedious reading, and Rodegra would have produced a better monograph if he had summarized some of this material. Rodegra lacks a gift for synthesis, and the book suffers at times from exasperating repetition. Moreover, the end-paper illustrations are primarily of antiquarian interest and do little to enhance the text.

HANNAH S. DECKER
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FRIEDRICH WILHELM GRAF. *Die Politisierung des religiösen Bewusstseins: Die bürgerlichen Religionsparteien im deutschen Vormärz; Das Beispiel des Deutschkatholizismus*. (Neuzeit im Aufbau, number 5.) Stuttgart: Frommann-Holzboog. 1978. Pp. 441.

Die Politisierung des religiösen Bewusstseins is a volume in the series "Neuzeit im Aufbau." The books in this series are concerned with society and ideas and aim at suggesting new lines of inquiry. Each volume consists of a treatment of the subject, a substantial collection of supporting documents, and an extensive bibliography. Friedrich Wilhelm Graf's volume conforms to this model.

The author raises the question of how the political philosophies of the early nineteenth century entered into the public consciousness, as expressed in the press and political clubs of mid-century, and finally resulted in political parties. What is required, he says, is "an analysis of the historical transformation process in which the original pure philosophical strife between liberal and conservative positions became a public question" (p. 18). The "transformation process," he believes, went forward through movements of religious protest at a time when industrialization had not yet generated a strong, politically active bourgeoisie. The central theme of the book is the exploration of this process as reflected in the German Catholic movement.

The German Catholic movement arose in 1844 out of the protest against the exhibition of the Holy Coat of Trier. Elements among the educated and lower middle classes and the artisans were its princi-

pal leaders and adherents. Its communities, formed outside of the church and beyond the control of the hierarchy, saw themselves as the fruits of a second Reformation, designed to realize better than the first a higher Christian life free from the constraints of unbending dogma and authority.

Graf shows how latent political feeling became articulated or implied in the movement's polemic. Thus the German Catholics could only feel enmity toward the restoration state that sought to integrate and dominate society through a statist royalism legitimized by Christianity. They gradually became a vehicle for the hitherto inarticulated "consciousness of opposition." As followers of Jesus, the original model of moral freedom, they were "an expression of the will to freedom." They perceived their movement as the means of implementing the idea of humanity, of creating a religious socialism that would overcome the egoism of the individual. In Graf's view German Catholicism thus fulfilled the longing of the dissatisfied who easily identified with the movement.

The consequences became apparent in 1848 when the German Catholics and those inspired by them played a conspicuous role. Although the German Catholic communities were not entirely in agreement on the revolution's objectives, they did conceive the revolution as "carrying through into politics what the second Reformation had formulated" (p. 140). The importance of the religious protest for the revolution, Graf concludes, was that it had made criticism of the system of the restoration "an affair of the people" (p. 165).

The book leaves the reader with some questions. Although Graf has shown that the movement stirred the consciousness of opposition among the leaders and spokesmen, it is not clear whether the followers sensed the political implications or whether their orientation remained primarily religious. The point needs clarification before one can suggest that the "partisan will" of the masses was aroused. Moreover, a single-dimensional explanatory model does not take into account the significance of other factors. Economic concerns, for example, have been an important inducement for collective political action in Germany as elsewhere.

Such questions aside, Graf's book is a provocative and imaginative analysis of German Catholicism and its significance. It offers fresh insight into the endlessly complicated problem of the rise of the revolutionary spirit and, in particular, throws light on the connection between political and religious protest in 1848. The collection of documents will make the book useful in the seminar, and the suggestiveness of the text will make it stimulating to the scholar.

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LOTHAR GALL. *Bismarck: Der weisse Revolutionär*. Frankfurt a/M: Propyläen Verlag. 1980. Pp. 812.

The publication in Germany of a major new biography of Bismarck is noteworthy quite apart from its merit. The famous statesman, once the object of scholarly as well as popular veneration, has been largely ignored in his country since the Second World War. Whenever he has appeared, it has usually been in the context of some secular political or social process to which he contributed: the development of an authoritarian tradition among his countrymen, the turn toward social imperialism, the attempt to suppress the labor movement, or the erosion of parliamentary institutions. The best-known biographies since 1939 have been written in other countries: the massive three-volume indictment by Erich Eyck in Switzerland, the scholarly life and times by Otto Pflanze in America, and the sparkling but crotchety *jeu d'esprit* by A. J. P. Taylor in England. The underlying assumption, moreover, of much of contemporary scholarship on Bismarck has been that his lifework must be understood as part of a long historical continuity culminating in the Third Reich. What distinguishes Lothar Gall's biography is not only that he has turned away from the preoccupation with political and social forces toward a renewed emphasis on the role of personality in history but also that he has been able to do so without the self-criticism with which many German scholars have come to view their nation's past. He belongs to a new generation, too young to have witnessed the rise of National Socialism, to have fought in the *Wehrmacht*, or to have joined the Hitler Youth. He is not burdened by the feeling of guilt that has weighed so heavily on those historians reaching maturity before 1945.

His biography of Bismarck, scholarly and reflective, represents rather a return to an earlier tradition of historiography with occasional echoes of Erich Marcks and even Arnold Oskar Meyer. First, this is a study of a man, not of a man and his times. Bismarck is always at the center of the stage; the age in which he lived is only the backdrop against which he practiced his statecraft. Second, there is little preaching about the baleful influence of the Iron Chancellor or about the connection between the Second and the Third Reich. The Bismarck in these pages is not a blind reactionary or ruthless Machiavellian, but a "white revolutionary" who recognizes the need for change. The national unification of Germany was historically inevitable, which means that it was also in a sense progressive. But Bismarck was not a magician pulling political rabbits out of the hat; he was a sorcerer's apprentice, unable to control the forces he had helped unleash. After 1871 the advocate of reform became a defender of the status quo, and his policies grew increasingly unimaginative.

tive, inflexible, and ultimately futile. He was not the author of his country's misfortunes and transgressions, but he was part of a generation that remained stubbornly blind to the dangers looming ahead. This is not a highly original interpretation, but it is clearly defensible, and Gall has defended it with learning, intelligence, style, and sensitivity.

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ALFRED KELLY. *The Descent of Darwin: The Popularization of Darwinism in Germany, 1860–1914*. Chapel Hill: University of North Carolina Press. 1981. Pp. 185. \$18.50.

In recent years historians have drawn parallels between German Social Darwinism—especially the variety touted by Ernst Haeckel and his monist followers—and Nazi ideology. Alfred Kelly resists this idea, arguing that “if Haeckel and the Monist League can be forerunners of Nazism, then so can most any thinker or organization” (p. 121). He also denies that Haeckel was the most important popularizer of Darwinism in Germany and would assign the distinction to Wilhelm Boelsche, by now a largely forgotten writer, but enormously popular in Germany around the turn of the century. Boelsche, however, was a spokesman for Haeckel and monism, and most of the substance of his popularity was derived from the doctrine of his mentor. To substitute the proselytizer for the prophet, as Kelly does, simply because Boelsche sold more books than Haeckel, is not convincing. As for Haeckel himself, “[he] ought not to be labelled a social Darwinist” (p. 114) but was really an ordinary liberal scientist without much interest in politics. Had the author been able to substantiate these statements, he might have reconstructed a major portion of German intellectual history. Kelly unfortunately is unable to distinguish between scholarship and invention, and when not compounding erroneous interpretations, his book offers abundant gratuitous assertions as well as disconcerting kaleidoscopic shifts in point of view. The evidence linking certain aspects of monism and National Socialism is overwhelming, and the author deliberately avoids it. Although he concedes Haeckel’s ideas and lifelong activities on behalf of Social Darwinism, he still insists that Haeckel did not really subscribe to them. After all, if Haeckel really “was a social Darwinist . . . why was he consistently attacked by conservatives as a danger to public order?” (p. 113). But this is bizarre and confused reasoning. Proto-Nazi Social Darwinism was not on the whole a phenomenon of the conservative right but was a special outgrowth of liberalism, positivism, science, and social democracy as well as an expres-

sion of the *völkisch* movement, which attempted to transcend political, ideological, and class polarizations. There was no unbridgeable gap between Social Darwinism and the left as Kelly seems to believe.

Absolving Haeckel, Kelly writes that the “occasional hints of anti-Semitism in Haeckel’s works are not racial, they are a spin off of his anti-Christianity” (p. 117), but not one citation is provided to support this. In fact, Haeckel explicitly defined the Jewish question as “racial” and not religious in an interview with Hermann Bahr, and in *The Riddle of the Universe*, Christ is denied his Jewishness because he was the illegitimate offspring of a Roman officer, of the “higher Arian [*sic*] race” who had seduced Mary. Other known racial antisemites like Heinrich Pudor and Ludwig Plate also found a congenial environment in the Monist League.

On a somewhat related matter, evidence linking Boelsche to Nazism is obscured. Buried in a footnote (p. 160, n. 79) is Kelly’s misleading reflection that it is difficult to know whether Boelsche supported the Nazis or not. But there is no mystery on this point because Boelsche defended Hitler’s Reich as the realization of monist ideals. An account of this appears in Kelly’s Ph.D. dissertation (pp. 230 and following), but here it has somehow vanished. Nor is Kelly’s discussion of Boelsche’s “erotic monism” enlightening. A fascinating idea, but deprived of its rich traditional and contemporary context, it is presented instead as the “Climax of Popular Darwinism.” Rather than beginning with Darwin (p. 48), erotic monism is traceable to Plato and Ficino, for whom love unified the universe: *amor nodus perpetuus et copula mundi* (Ficino). And more than a vague symptomatic crack in nineteenth-century “puritanism” (p. 56), Boelsche’s “paneroticism” should be measured against the popularity of love and sex in the later nineteenth century, taking into account such movements and individuals as *Freikörperkultur*; Max Ferdinand Sebaldt’s occult *Sexualreligion*; the contribution of Fidus, the eminent proto-Nazi artist influenced by Boelsche; Hans Jaeger’s program of sexual freedom; and the free *À Rebours* love of the Decadents. Kelly separates Boelsche from all these currents of thought and activity.

It would be difficult to see, in the light of its many distortions, lacunae, and careless contradictions, what this work can add to an understanding of the subject.

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GEOFFREY G. FIELD. *Evangelist of Race: The Germanic Vision of Houston Stewart Chamberlain*. New York: Columbia University Press. 1981. Pp. x, 565. \$25.00.

Houston Stewart Chamberlain's *Foundations of the Nineteenth Century*, as Geoffrey G. Field has demonstrated, was "the literary fad" (p. 226) of the turn of the century in Germany. Not just a fad, Chamberlain's work was also a synthesis of Germanic ideology and racist ideas that resounded throughout the Nazi era. In 1955 Jean R  al, in an essay written for UNESCO's collection titled *The Third Reich*, summarized Chamberlain's ideas under the rubric "the religious conception of race." Field goes further than R  al by excavating the literary, academic, and political contexts in the midst of which Chamberlain lived and wrote.

Chamberlain did not spring to life a full-blown fascist. In his youth he was a Gladstonian liberal as well as an admirer of German cultural nationalism, a Wagnerite who also appreciated other composers, and, at one time, a stockbroker, despite his loathing of commerce. Buffeted by the intellectual currents of his day and driven by the inner forces of his personality, Chamberlain created an ever narrower, though seemingly erudite, version of world history. He was both a product and a prophet of his times. On the one hand, Chamberlain "owed his success to the way his book captured the mood of his contemporaries" (p. 276); on the other hand, he originated "the most sustained and ambitious attempt before 1933 to formulate a Germanic world view" (p. 448). Field cautions us that Chamberlain, although he lived long enough to embrace Hitler, was a creature of the Wilhelmian age. He was a monarchist for most of his days, an aesthete, and a sort of Christian. Nonetheless, Chamberlain contributed heavily to Germany's reserve of antisemitism, and Field proposes to demonstrate how the pre- and post-World War I eras were connected by examining the gradual process "whereby large numbers of educated Germans became accustomed to racialist rhetoric and forms of arguments and desensitized to the plight of Jewish fellow citizens" (p. 6).

Field asserts that biography is an especially good vehicle for describing the many associations and attitudes that link an individual with his or her time. Although he contributes no novel ways of conceptualizing these links, he has excelled in integrating extensive archival research on Chamberlain with wide reading of recent general scholarship on this period. First, he discusses not only the diverse responses of *v  lkisch* politicians and Kaiser Wilhelm himself to the *Foundations* and other works of Chamberlain, but also those of German liberal Protestants, such as Adolf von Harnack, and the German Jewish press. He affirms the view that, although parliamentary antisemitism may have declined before the war, the diffusion of antisemitism throughout society increased. Second, he traces disputes among academicians, such as anthropologists, philosophers of science, and theologians, about issues of national

identity and suggests specific cases in which the distance between scholarly controversies and vulgar racist journalism was rather small. Third, he shows Chamberlain at work as a publicist. We are given a glimpse of his "method" of research, namely, the copying of quotations each morning to fit the arguments to be expressed in the day's work. We learn that Chamberlain managed to appear persuasive rather than combative and that he capitalized on thorny scholarly controversies by appearing to transcend while in reality distorting them.

Although he writes biography, Field resists psychobiography. Apart from introductory material on Chamberlain's childhood, Field does include one chapter on "private life." Chamberlain seemed dignified and refined to his friends, and timid and anxious to himself. He suffered two major nervous breakdowns, and he dreamed cruel, paranoid fantasies about Jews. Field summarizes the avenues of psychological analysis suggested by other scholars, but offers none of his own.

Chamberlain's relationship with the Wagnerites of Bayreuth concludes the study. Not always the perfect Wagnerite, he antagonized Cosima Wagner in several ways. Nonetheless, his pre-eminence as an author and war propagandist, Field argues, sustained Bayreuth during the immediate postwar period. Bereft of his king, Chamberlain saw the need for a new style of politics and took a not unwilling Bayreuth with him. Enchanted by Hitler's preference for Wagnerian opera, Chamberlain publicly proclaimed his allegiance to the Nazi party, a loyalty unchanged at his death in 1927.

Scholars hoping to understand the intellectual milieu of antisemitism in the Wilhelmian period will appreciate the richness of this excellent study.

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GARY D. STARK. *Entrepreneurs of Ideology: Neoconservative Publishers in Germany, 1890-1933*. Chapel Hill: University of North Carolina Press. 1981. Pp. xii, 327. \$26.50.

In his preface Gary D. Stark sets forth his intention. "I chose to explore the history of the German neoconservative movement from the perspective of the publishing industry and book trade." Advocates of the radical, right-wing position were usually publicists and writers, and they found ready backing from publishing houses such as the Eugen Diederichs Verlag of Jena, Julius Lehmann of Munich, Gerhard Stalling (Oldenburg), Heinrich Beenken (Berlin), and the Hanseatische Verlagsanstalt of Hamburg. These firms were independent of any political party and prided themselves on their reputation for respectability. Sometimes they diverted

their profits from general publishing to the support of neoconservative causes, but in no sense were they part of the right-wing lunatic fringe. Eugen Diederichs made this abundantly clear: "My publishing house rests entirely on my personality and my personal opinions," and he promoted many works "simply because I personally wanted to know something more about that particular topic" (p. 34).

What, then, attracted these entrepreneurs to the neoconservative movement? Stark goes to great lengths to clarify this, but one comes away with the impression that neoconservatism was less a movement than a reservoir of contradictory attitudes. It repudiated the Bismarckian alliance between the landed aristocracy and heavy industry, was culturally elitist and pessimistic, and was antimaterialistic and antimodernistic. No two devotees of the cause, however, could have differed more widely than the leading neoconservative publishers from Jena and Munich.

Diederichs had a distinctly philosophical turn of mind, influenced greatly by the writings of Burckhardt, Nietzsche, and Lagarde. Among his distinguished authors were Maeterlinck, Bergson, Tolstói, and Kierkegaard. Because he was sympathetically inclined toward pantheism and mysticism, he published the works of Rudolf Steiner and other theosophists as well as those of many traditional medieval mystics.

By contrast, Lehmann was an activist who joined countless ultranationalist organizations, including the Free Corps, which ousted the communists from Munich. He favored publishing works on practical subjects like medicine and science, augmented by eugenics, racial biology, and antisemitism. He was the familiar type of pan-German racist who found the Nazi movement congenial and had no difficulty accepting Hitler's ideology.

I would have preferred this book to have dealt primarily with the intriguing and significant figure of Eugen Diederichs rather than the broader topic of neoconservatism. Stark, however, chose to investigate five publishers in a set of case studies. I am a bit dubious about the statement that neoconservatism laid the "groundwork for the triumph of Nazism" (p. 62), but Stark effectively demonstrates the various ways in which authors and publishers undermined public confidence in the Weimar Republic. He admits that the evidence regarding his subject is "spotty" and "fragmentary" because of the ravages of war. For example, he tells us that publishers like Diederichs and Lehmann lost 90 percent of their sales during the First World War and that nearly all of their remaining capital was consumed by postwar inflation. Yet there is no attempt to analyze these economic catastrophes in terms of how they may have influenced the inclination and ability of the reading public to procure new books.

Stark's writing is weakened somewhat by repetition, but his research is thorough and accurate.

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FRIEDHELM BOLL. *Frieden ohne Revolution? Friedensstrategien der deutschen Sozialdemokratie vom Erfurter Programm 1891 bis zur Revolution 1918.* (Forschungsinstitut der Friedrich-Ebert-Stiftung, Politik- und Gesellschaftsgeschichte, number 8.) Bonn: Verlag Neue Gesellschaft. 1980. Pp. 296.

The title of Friedhelm Boll's book is somewhat misleading. Although he does analyze the changing attitude of German Social Democracy toward international peace, his work goes far beyond that rather limited, if important, issue to consider the more general and more interesting question of the party's position on the use of force. In the process he covers a great deal of territory that is familiar to most students of German history, but he does so from a point of view that offers numerous new perspectives and insights.

Boll's approach is at least in part a product of his participation in a study group on peace research, and he makes periodic and often quite suggestive efforts to relate his subject matter to current discussions in that field, but most of his book is straightforwardly historical in its orientation. In his treatment of the period before 1914 Boll argues that the Social Democratic leaders dealt with the troublesome question of the use of force by developing the concept of the *Aufklärungsrevolution*, that is, a revolution that would result from the gradual conversion of the masses and would require the use of force only in defense of democratic rights. This provided a justification for the SPD's practical work in the area of domestic politics but, unfortunately, did not explain how it would respond to the outbreak of an international conflict. Despite the party's often radical rhetoric on the peace issue, it was never able to develop a concrete plan specifying how it would react should its efforts to prevent war fail. Lacking such a plan, domestic political considerations became increasingly dominant during the immediate prewar years, thus setting the stage for the majority of the Social Democratic leaders to accept the regime's foreign policy during the war in the hope of achieving significant internal reforms.

In the two-thirds of his book that he devotes to the period after August 1914, Boll is extremely critical of the Majority Socialists. In his view they vastly overestimated the chances of gaining concessions from a victorious regime and so thoroughly accommodated themselves to the attitudes of their erstwhile opponents on the right that they were unable to recognize the very real opportunities for

fundamental change provided by the Reich's defeat in 1918. He also has little praise for the Spartacists, who succeeded in isolating themselves through their unrealistic "revolutionary maximalism." He reserves his approbation primarily for the Independents, who, in his opinion, prepared the way for the largely nonviolent upheaval of November 1918 and pursued a policy that, given the cooperation of the Majority Socialists, might well have led to the establishment of a firmly based democracy with a conciliatory foreign policy.

Potential readers should not be deterred by the fact that this book is Boll's dissertation. Despite occasional lapses into somewhat pedantic language that can hardly be avoided in this genre, it is generally well written and characterized by a refreshing originality that, though not always completely convincing, is consistently stimulating.

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CARL-LUDWIG HOLTFRERICH. *Die deutsche Inflation, 1914–1923: Ursachen und Folgen in internationaler Perspektive*. New York: Walter de Gruyter. 1980. Pp. ix, 360. \$58.00.

This is a pioneering and major work of economic history that must be consulted by anyone interested in the German inflation and the international economy of the interwar period. Of course, nearly any serious historical study of the German inflation could merit some such comment since there are no significant general studies of the German inflation except those produced by economists. Furthermore, the works of Costantino Bresciani-Turroni and Frank D. Graham first appeared a half century ago, whereas the 1964 study of Karsten Laursen and Jørgen Pedersen offers an important if very unhistorically argued Keynesian interpretation but very little new data. The virtues of Carl-Ludwig Holtfrerich's book, however, do not derive from the advantages of temporary monopoly but from major research breakthroughs, the novelty and originality of important portions of his argument, and the fact that he has set the much-needed framework for a modern discussion of the German inflation.

The study is divided into three parts. The first, perhaps of greater interest to economists than historians, is devoted to the various inflation indicators and discusses the available data on prices, cost of living, money supply, and liquidity. Here, as throughout the study, the author devotes serious attention to the wartime period, in which the inflation had its origins, and subjects the statistics to a critical but balanced assessment. The second section deals with the causes of the inflation, specifically, the financial policy of the Reich, the credit policies of

the Reichsbank, and the role of reparations. Holtfrerich insists upon the role of political factors in determining the huge budget deficits that fueled the inflation as well as upon the role of political events in determining the major short-term fluctuations in the exchange rate and inflationary expectations. The concluding portion of the book discusses the effects of the inflation with respect to economic growth and its domestic and international redistributive functions and argues that the inflation promoted economic growth, greater equality of income distribution, and the German economy relative to that of its international competitors.

Holtfrerich's arguments are underpinned by important original research. He explains for the first time how it was that the hyperinflation that began in the summer of 1922 did not begin earlier despite increasing governmental budget deficits. The answer lies in the immense influx of foreign, especially American, speculative capital placed in Germany between 1919 and 1922 in the expectation that the mark would recover its strength and that one could make a "killing" by being repaid in marks. Thanks to the hyperinflation, Germany became the recipient of what was tantamount to a free loan that helped keep it afloat during the crucial years after the war. At the same time, Holtfrerich shows that the leaders of the Reichsbank, including its much-maligned president, Rudolf Havenstein, were not quite as witless as appears in the standard accounts. Their alleged adherence to the balance-of-payments theory of the inflation between 1919 and 1921 was largely for public consumption, while they privately and persistently warned the government that the budgetary deficits and the accompanying discounting of treasury bills were providing the chief fuel for the inflation. They were not fanatical opponents of the quantity theory.

In the last analysis, Holtfrerich argues, the competing economic theories of the inflation were functional and depended upon the priority one assigned to stabilization as opposed to inflationary reconstruction and crisis management. Faced with persistent dangers of domestic revolutionary upheaval and reparations demands that were first indeterminate and then very substantial, Germany's political and economic leaders chose inflation as a means of maintaining employment, reviving production, recapturing world markets, and resisting the Allies. For political and economic reasons, inflation appeared to be the least harmful alternative. Even if one disagrees, Holtfrerich's insistence that "currency stability at any price in the realm of economics is just as naive a policy a victory at any price in war" (p. 297) is most appropriate in the current environment.

This is not to argue that the German inflation was "no worse, so to speak, than a bad cold" (*AHR*, 86

[1981]: 361). Neither this reviewer, whose appreciation of the German inflation was so characterized by Charles Kindleberger, nor Holtfrerich would suggest such exercises as the ideal way for a state to eliminate its internal and external obligations. The real issue is how one is to weigh benefits against damages in the context of the domestic and international conditions of the post-1918 period. Bresciani-Turroni's pessimistic view of stabilization possibilities in his unpublished reports to the Reparations Commission in 1920–21 sheds a curious light on the harsh hindsight of his later book and shows how important is the corrective that Holtfrerich has provided. This study demonstrates that an effort to present a value-free and more historicist discussion of so complex a historical phenomenon as the German inflation can lead to many refreshing "thoughts out of season" indeed and that economists can contribute much to our understanding—and their own—by systematically integrating political variables into their analyses.

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MANFRED J. ENSSLE. *Stresemann's Territorial Revisionism: Germany, Belgium, and the Eupen-Malmédy Question, 1919–1929*. (Veröffentlichungen des Instituts für Europäische Geschichte, Mainz, Universalgeschichte, number 94.) Wiesbaden: Franz Steiner Verlag. 1980. Pp. 229. DM 48.

Manfred J. Enssle examines the connection between two relatively neglected problems inherited from World War I—German marks in Belgium and the provinces of Eupen and Malmédy on the Belgian-German border—and Stresemann's more frequently studied revisionist policies. The early chapters of Enssle's book cover the background to later revisionism and end with the negotiations—discussed in the final 9 pages of chapter 4—concerning a Belgian-German commercial accord signed in 1925. The next 100 pages focus on Belgian-German conversations to promote the return of Eupen-Malmédy to Germany and the relationship of this undertaking to Stresemann's broader pursuit of revisionism through his *Locarno-Politik*. To this Enssle appends a retrospective chapter that lists eight major conclusions to be drawn from his study (pp. 187–204).

In the chapters concerning Locarno and Eupen-Malmédy, Enssle argues that the two lines of policy were intimately linked (preface, pp. 84–87), that they constituted a revision of the territorial as well as of the political and economic status quo, that Stresemann's pursuit of the retrocession of the two provinces violated his promise to guarantee at least the territorial status quo in the west (pp. 97, 98, 100,

and n. 110), and that ultimately Stresemann's aggressive pursuit of the western territorial change poisoned the diplomatic atmosphere and violated the trust that French and British statesmen had begun to develop in Germany (pp. 198–204).

Enssle's presentation overlooks some important documentation. For instance, the papers of British Ambassador Lord D'Abernon and of Belgian Foreign Minister Paul Hymans establish clearly that the "Gaus interpretation" of the Locarno Pact did not remain "unclear until early August [1925]" (p. 107) but was from the outset a part of the carefully chosen phrasing of Germany's proposal to guarantee the "present territorial disposition"—and not the status quo—on the Rhine. Moreover, Hymans in particular did not regard the proposal as a "German accommodation to the *status quo*" (p. 82). In addition, the papers of Hermann Pünder contain a report of the jurists' discussions at Locarno, which Enssle asserts were not recorded (p. 111). Particularly important, this shows that British jurist Cecil Hurst clearly understood that the Germans were reserving the right to pursue peaceful territorial change under the terms of Locarno. All of this documentation is cited in my article, "Germany and the Eupen-Malmédy Affair, 1924–1926: 'Here Lies the Spirit of Locarno'" (*Central European History*, 8 [1975]: 227, 230).

In criticizing that article, Enssle notes that "fundamental differences of interpretation" (p. 117n.) separate us. This is true, but on many points we also agree. For example, Stresemann's interest in Eupen-Malmédy was both revisionist and an integral part of his overall policy—that is why I chose to quote him in the title of my article. We agree also that the Locarno accords did *not* guarantee the territorial status quo of the Rhineland. As Enssle records, however, Stresemann acknowledged the permanency of the loss of Alsace-Lorraine (pp. 110, 124) because no possibility of its retrocession to Germany existed. As this distinction illustrates, Stresemann pursued only those objectives he thought had some chance of realization. Enssle amply shows that the Belgians, and even the French upon occasion, gave Stresemann good reason to believe that the retrocession of Eupen-Malmédy belonged in this category of the possible. As a diplomatic arrangement, it was certainly more likely than had been the return of Alsace-Lorraine to France prior to 1914. For their part, the Germans had never intended that the security pact negotiations constitute a guarantee of the status quo, west or east, a fact that D'Abernon and Hymans had understood as early as February 1925. For Belgium, the Eupen-Malmédy question always came down to this: Would the advantages of an exchange outweigh the disadvantages, including possible hostility from France to the whole idea? The final Belgian answer to this question was clearly

no, both in 1926 and again in 1929, as Enssle shows. But it came not because of any sudden awakening to the dangers of the precedent of territorial revision, which was the recognized German objective from the beginning. It came because of a change of circumstances that tipped the balance sharply to the negative, a change involving personnel and finances in both Belgium and France.

Finally, my most fundamental disagreement with Enssle's presentation derives less from reservations concerning particular issues or documentation than from the differing conceptions we have of the interwar system established by Versailles. A. W. DePorte has recently written that "the seeming restoration of the old European system after 1918—the touchstone of normalcy for much American thinking about Europe—was artificial, unnatural, and transitory . . ." (*Europe between the Superpowers* [1979], p. viii). I find the remark telling. Many statesmen of the interwar period, including Stresemann, recognized the transitory character of a system that did not even have the wholehearted support of the powers that had established it. For those who so perceived, revisionism was necessary not only for Germany but also for Europe, in order to channel change into acceptable paths. Stresemann's revisionism was aimed not at unlimited *Weltpolitik*, as Enssle asserts (p. 82), but at greater stability through negotiated, peaceful change. The Locarno statesmen disagreed about the relative positions of their own nations within the emerging system, but they agreed about the need to adjust that system. Enssle finds Stresemann's promotion of change destabilizing. By contrast, I see it as a search for—and an acceptance of—limits in the quest for a new stability. Because Stresemann saw in it the only hope for true German recovery, he valued the process of cooperative adjustment more than any single objective of policy. His *Locarno-Politik* represented that cooperative process, Eupen-Malmédy but one objective.

This book is not the last word on Eupen-Malmédy in the diplomacy of the 1920s. Enssle has advanced the story, but we still have much to learn.

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RICHARD BREITMAN. *German Socialism and Weimar Democracy*. Chapel Hill: University of North Carolina Press. 1981. Pp. xii, 283. \$20.00.

Behind its rather elastic title, *German Socialism and Weimar Democracy* is a book primarily concerned with "the interaction between the SPD leaders and the parliamentary system" (p. 7), that is, with the parliamentary and especially the coalition policies of a restricted Social Democratic elite. This is a limited

yet certainly a significant subject, and, as the dust jacket says, this is "the first analysis in English." Founded on a wide range of archival and published materials and argued in a careful and responsible manner, the book should go straight onto the shelf of standard works. Yet it is in some ways disappointing.

Richard Breitman's evaluation of his subject is balanced and perceptive: he sees the party leaders as constantly influenced, on the one hand, by the necessary dynamics of multiparty democracy and a determination to preserve the republic and, on the other hand, by residual ideological optimism, the constraints of interest-group politics, and an inadequate understanding of the economy. He presents intelligible brief accounts of such tangled subjects as the fight over the *Erfassung der Sachwerte* (1921–22) and the implications of the theory of organized capitalism. By simultaneously treating Prussian and Reich developments from 1928 onward, he enables us to see how they were connected in the minds of the party leadership. There are numerous nuances and new bits of information gleaned from the archives.

But the book could have been better. For one thing, given its brevity (there are 194 pages of text), the choice of a largely narrative structure is questionable: a great deal of valuable space is devoted to telling us what we already know, with minor differences. At the same time, the argumentation is obscured; for the most part, it emerges clearly only in the introduction and the conclusion. The work is often weak in distinguishing what it has to say from what others have said and in making its points with verve and incisiveness. This is partly because the author has chosen not to conduct an argument with others, even implicitly, but to tell his own story—a story that is well conceived but not really pointed in any way. The author's contention that the failings of the SPD owe more to circumstances and long-term tendencies than to egregious errors or dominant personalities—which is in itself surely correct—also works in part to mute the drama and color of a vivid and controversial slice of history.

Finally, although the book effectively elucidates socialist coalition politics in the narrow sense—how choices were determined by the leaders' conception of what was possible in the parliamentary framework—there were other forces working on the leadership that were more important than they appear here. One element deserving much more weight is the impact of trade-union thinking on SPD policy formation, indicated only intermittently and in global terms here. The same is true of the internal stresses of the party, which made up a more important part of the milieu within which the leadership operated than one would guess from this book. Until such things are taken into consideration,

our understanding of the Weimar SPD's approach to parliamentary work and coalition policies will have its gaps.

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DETLEV PEUKERT. *Die KPD im Widerstand: Verfolgung und Untergrundarbeit an Rhein und Ruhr, 1933 bis 1945*. (Düsseldorfer Schriften zur Neueren Landesgeschichte und zur Geschichte Nordrhein-Westfalens, number 2.) Wuppertal: Peter Hammer Verlag. 1980. Pp. 460.

This is an often fatiguing book to read and is in several respects ticklish to review. It deals with a subject, the resistance to Nazism inside Germany, that was at best a feeble affair and that in some of its better-known forms, whether right or left wing, was not particularly inspiring or attractive, especially to non-Germans. And it failed overwhelmingly. Yet here is a tome of some 460 pages (but no index) admirably devoted to the anti-Nazi resistance in a single region of Germany and within a party whose most characteristic attitude of resistance was to wait until the Nazis destroyed themselves and, that failing, to lie low.

To be charitable, the relentless attention to minutiae in this work may be partially defended as appealing to local and provincial interests. The detail is also supposedly justified because it brings to light (as the cover proclaims) "the astonishing scope and heroic daring (*Wagemut*)" of Communist rank-and-file opposition to Hitler. Detlev Peukert stresses that only by paying respectful attention—at long last—to the major contributions of this mass resistance can the present-day citizens of the Federal Republic master their own history.

Die KPD im Widerstand is not, initial suspicions to the contrary, an exercise in hagiography, although the author's instincts seem to be in that direction. He has read and researched widely, and he works diligently at balanced judgments. He is hard on the leadership of the KPD, and his critical yet measured remarks concerning official East German histories of these years form a leitmotiv of the book.

Aside from the thorny, politically sensitive issue of giving Communist resistance its due, a detailed volume of local history that offers insights into larger issues is especially welcome these days. But unfortunately this volume fails to make a major contribution in this regard: its abundant detail elaborates upon rather than substantially reinterprets the most important and familiar generalizations about the KPD. Similarly, the imaginative use of nonliterary evidence, offering insights into the lives of the inarticulate, that is so often an aspect of the best local studies, plays only a small role here.

My further reasons for a sense of dissatisfaction and impatience have to do with matters already touched upon and that are perhaps too "political" to deal with properly, especially in a short review. Yet, even at the risk of misinterpretation, I cannot wholly ignore them. The author is a young but already productive scholar, and I have already made clear that he is not a simpleminded partisan. Nevertheless, a kind of scholasticism pervades this volume, replete with axioms that could have been scrutinized more imaginatively. Fascism provides for the author a convenient absolute evil; antifascism, however flawed in its particulars, is thus in some ultimate sense heroic. In the same way, he seems to accept as axiomatic that the working class is a vehicle for the forward march of humane values and rational conduct. The KPD, because of its antifascism and its organic connection with the working class, is thus by definition on the side of the angels. The author appears to consider it absurd to suggest that the KPD and NSDAP morally resembled one another. Yet Stalinism in the 1930s was at least as brutish in form as Hitlerism and was responsible, at least until 1939, for many more deaths, indeed for organized murder on an unparalleled scale.

The KPD enthusiastically associated itself with the nightmarish inhumanities of Stalin's rule. This association, and many kindred attitudes, was fatal not only to proletarian solidarity but also to the potential solidarity of those who remained attached to liberal-democratic values (did this include the KPD?). Could one not, then, plausibly point an accusing finger at Russian communism and its allied parties not only for providing a decisive stimulus to the growth of fascism but also for providing certain models in the techniques of unscrupulous mass mobilization—and mass murder?

Communist "heroism," which I personally do not reject out of hand, nevertheless takes on some exceedingly ambiguous hues in the context of such considerations. The author repeatedly emphasizes the need for Germans to grapple with unpalatable subjects if they are to master their history. Could he not, in the luxury of 430 pages often devoted to a laborious examination of inconsequential matters, have heeded his own advice?

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REINHOLD W. WEBER. *Die Entstehungsgeschichte des Hitler-Stalin-Paktes 1939*. (Europäische Hochschulschriften, third series, Geschichte und Ihre Hilfswissenschaften, number 141.) Frankfurt a/M.: Peter D. Lang. 1980. Pp. 305. 45 FR.

Reinhold W. Weber's book attempts to explain the failure of the 1939 Anglo-French-Soviet alliance

talks and the origins of the Nazi-Soviet Pact. It is highly traditional in its approach, but its careful analysis and good use of a variety of sources justify going over this well-traveled ground again. Focusing on Britain, the Soviet Union, and Germany, the book says some new things about all three, and its general thrust—that the failure of the Anglo-French-Soviet talks and the success of the Nazi-Soviet Pact logically grew out of the policies of these three powers—seems sound. Although not a ground-breaking study, the book is a worthwhile contribution.

In the first part of the book Weber relies on cabinet papers, published sources, and to a lesser extent Foreign Office correspondence to draw a coherent picture of British policy after the German occupation of Prague. Attacking a still controversial issue head on, he argues impressively that Neville Chamberlain was forced by parliamentary and public opinion to take a stronger anti-German stand in late March 1939, that a wide-ranging agreement with Germany remained the goal of his foreign policy, and that he resisted serious alliance talks with the Soviet Union and pursued them halfheartedly because he feared that they would frustrate his real aim. Some of his best evidence comes from Chamberlain's public speeches, which have been somewhat neglected since the opening of the archives but which leave no doubt of his continuing interest in further negotiations with Germany. It is equally clear that Soviet Ambassador Ivan Maiskii had no illusions about Chamberlain's real attitude toward the talks, although we shall see that the clear implication that Britain's halfhearted attitude was *responsible* for their failure is less defensible.

With respect to Germany, Weber's reliance upon Italian documents and German government instructions to the press gives a somewhat more sketchy picture of German foreign policy during these critical months. His strategy is sound; earlier works such as Theo Sommer's study of German-Japanese relations have shown that the Foreign Office archives do not nearly provide a complete picture of German policy after the advent of Ribbentrop. Hitler had clearly begun thinking about an arrangement with Moscow by early May, when the German press was ordered to halt all attacks on the Soviets. Such a deal was an alternative to a German-Italian-Japanese alliance against both the Soviet Union and the Western powers. Weber argues that by July Hitler had determined to attack Poland in any event, citing Ribbentrop's repeated assurances to Italian diplomats that the Western powers simply would not intervene. Here he probably goes too far: Hitler seems to have recognized the necessity of closing a deal either with Japan or with the Soviets before beginning war. Weber also neglects Berlin's economic interest in a pact with Moscow. Although

he frequently refers to the German-Soviet economic talks that had begun in the spring and were concluded just prior to the political agreement, he never analyzes their content in any detail. Actually the deal with Stalin was as much an economic as a political necessity for Hitler. He could not have risked war with the West without Soviet raw materials and secure access to other key imports from the Far East.

Weber's analysis of Soviet policy is more questionable. He clearly believes that the Soviets were sincerely interested in an alliance with Britain and France until the fall of Litvinov in early April; the author's description of Molotov's endless stalling after that leads inevitably to the conclusion that Molotov was merely waiting for the Germans to step forward with an appropriate offer of their own. The missing man in this picture is Joseph Stalin. Undoubtedly Litvinov and Ambassador Maiskii were sincerely working for an alliance with the Western powers in March and April, but what about Stalin? Was it Chamberlain's halfheartedness that caused Stalin to turn toward Germany in preference to the West—as Weber clearly implies—or was it Stalin's own aversion to an alliance only too likely to involve the Soviet Union in a war he desperately feared? The intuitive analysis of Adam Ulam, who has argued that Stalin turned to Germany in 1939 to avoid war—and above all to make sure that war would not begin on Soviet soil—seems more persuasive on this point. The Anglo-French-Soviet talks failed because neither Chamberlain nor Stalin really wanted an alliance. If Stalin's real interest was peace, only Hitler could give it to him. Still, Weber's important work should inspire other doctoral candidates to take on broad and controversial topics.

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GERHARD L. WEINBERG. *The Foreign Policy of Hitler's Germany: Starting World War II, 1937–1939*. Chicago: University of Chicago Press. 1980. Pp. xii, 728. \$44.00.

This second volume of Gerhard L. Weinberg's history of the foreign policy of Hitler's Germany will not disappoint readers who have awaited it with eager interest since the appearance of the first volume, *Diplomatic Revolution in Europe, 1933–1936* (1970). Weinberg here continues his detailed analysis of the ideas, policies, events, and negotiations that shaped "the key choices and decisions" leading to World War II. Again the scope is impressively far ranging, encompassing the perspective not only of the German leadership but also of leading figures in many other countries. Approximately half of this lengthy volume is devoted to diplomatic interactions

in critical arenas: Britain and France, the Civil War in Spain, the Sino-Japanese War, Eastern and Southeast Europe, the Middle East, Italy and Austria, and the Western Hemisphere. The remaining half is devoted to the crisis over Czechoslovakia and the period preceding the outbreak of war in 1939. Despite some retracing of events necessitated by the geographical organization of the first half of the volume, the overall direction and argument remain clear. Weinberg holds steadfastly throughout to views expounded in the earlier volume. Against those who would highlight "appeasement" or provocation by others, Weinberg maintains his emphasis on "the initiative of Germany as Europe's most advanced and powerful industrial and military power" and pursues patiently his demonstration that Hitler's goals and policies remained consistent, deliberate though flexible, and persistently hostile to diplomatic accommodation on any basis short of a free hand for Germany in Central and Eastern Europe, with the ultimate aim of depopulating the latter to make room for a German empire dominant in Europe and the world.

Weinberg displays again that unmatched grasp of the enormous documentary and published literature that led reviewers of the earlier volume to acclaim it "a superb piece of research," "masterly," "social and intellectual history at its best." Nonetheless, as is already evident from Norman Stone's review in the *New York Review of Books* (April 30, 1981), this volume will not put an end to the tiresome polemics surrounding A. J. P. Taylor's controversial interpretation of Hitler, even though Taylor acknowledged the barrenness of the dispute (*Journal of Modern History*, 44 [1972]). Weinberg may be partly to blame for passing over too hastily in this volume certain fundamental points previously treated. Thus if Stone asserts, erroneously, that there is "absolutely no hard evidence" that Hitler's economic and military planning until 1939 was geared to *Blitzkrieg* rather than protracted war, Weinberg might be faulted somewhat for merely referring readers to his first volume for specific references on this important point. Still, the evidence supporting the soundness of his position, within the frame of reference of diplomatic history, is overwhelming.

Observant readers will also find again here those acerbic remarks, relished or deplored according to one's standpoint, with which Weinberg dismisses those of opposing views: "The idea that the historian can see better by walking on his hands instead of on his feet has the charm of novelty, but is apt to lead to misconceptions" (p. x). Unfortunately, it is not clear what constitutes "walking on one's hands." Foolish denial of "hard evidence" would certainly qualify, but one hopes that Weinberg does not also intend to dismiss all those who may see other forces at work in history than the "key choices and deci-

sions" of governmental and military figures at a particular moment. In this context, Weinberg may claim too much: a "comprehensive treatment of international relations and the origins of World War II," or still more, of "how the world came to be involved in a massive conflagration for the second time within a quarter of a century," calls for more attention to the multiple social, economic, political, and technological developments bearing on that problem than this work provides. But in its more modest goal of offering a contribution toward our understanding of that problem from the particular perspective of diplomatic history, this work is superbly done.

BERENICE A. CARROLL
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HELMUT GRIESER. *Die ausgebliebene Radikalisierung: Zur Sozialgeschichte der Kieler Flüchtlingslager im Spannungsfeld von sozialdemokratischer Landespolitik und Stadtverwaltung, 1945–1950.* (Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte, Beihefte, number 69.) Wiesbaden: Franz Steiner Verlag. 1980. Pp. viii, 185. DM 38.

Of the challenges to a revival of German democracy after 1945, none appeared more intractable than the integration of millions of ethnic Germans from Eastern Europe into the shattered economy of the three Western zones. Despite the significance of this issue, historical research has tended to focus on the diplomatic background of the division of Germany or, at best, on studies of the legal status of the expellees. Helmut Grieser has done much to remedy this deficiency in his case study of the expellee community in Kiel, where a high density of displaced persons requiring social assistance added a critical dimension to the problem of rebuilding a widely destroyed city.

The question of why the expellees failed to succumb to the lure of political radicalism provides the book's leitmotiv. Ethnic groups forcibly driven from their homeland and consigned to camps surrounded by an indifferent population readily become breeding grounds for revolutionary activism. Yet, after describing the extreme deprivation of camp life, the author goes on to explain the failure of extremist movements to take root. For example, the Communist party, above all others, expressed concern over the expellees' living conditions, but, because of its reluctance to reject unequivocally the Oder-Neisse frontier and to demand from the Allies a recognition of a right to one's homeland, the party forfeited whatever advantages it might have had. The author perceptively concludes that the Cold War tended to nullify the political consequences of

the tension between the expellees and their reluctant hosts.

Although he does not refer specifically to Roberto Michels, Grieser's analysis confirms the theory that political parties tend to become closed corporations dominated by an oligarchy resistant to change. As a case in point, the Social Democrats could not see beyond the parochialism of their functionaries and so failed to develop the electoral support commensurate with their ideological appeal among the expellees. The camps were simply left to govern themselves as enclaves, and local elites never seriously contemplated seating expellees on municipal councils or other governing bodies. The founding of the Federal Republic in 1949 was followed a year later by the organization of an expellee party, which offered to those segregated into camps an opportunity to compete politically. The new party served as the structural means through which Germans displaced from the East could begin to play a role as citizens in a new political system—a historical contribution to which Grieser gives full credit.

At the risk of caviling, the author's practice of using the words *refugee* and *expellee* interchangeably is disconcerting in that the former implies some degree of choice and the latter only coercion as a motive for leaving one's homeland. Indeed, the meaning of the population transfers in Eastern Europe and elsewhere depends upon the choice between these two words. Otherwise this study is a contribution to German historiography and to an understanding of a global tragedy of our time.

JAMES H. WOLFE

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FRIEDRICH VON RUMMEL. *Franz Ferdinand von Rummel: Lehrer Kaiser Josephs I. und Fürstbischof von Wien, 1644–1716*. (Schriftenreihe des Instituts für Österreichkunde, Österreich Archiv.) Munich: R. Oldenbourg Verlag. 1980. Pp. 163. DM 28.

Franz Ferdinand von Rummel owed his appointment as tutor to Archduke Joseph to Hippolito da Pergine, a strict Franciscan much in favor with Leopold I, and to his family's long service to Empress Eleanor's Pfalz-Neuburg connections. His elevation in 1706 to the episcopal see of Vienna he owed entirely to Joseph's affection for a childhood mentor. The Rummels had risen from estate managers and gamekeepers into the ranks of the professionally trained bureaucratic elite and only recently into the petty landed nobility. The parts of the monograph dealing with family background provide an interesting example of successful family strategy in the seventeenth-century empire. Friedrich von Rummel, the author and a descendant of one branch of the family, manages on the whole to

restrain his enthusiasm for ancestry and to give a simple, factual account. His intensive archival research has revealed some useful material in the Pfalz-Neuburg archives, now part of the Bavarian state archives in Munich. Unfortunately he found very little direct correspondence to or from his subject. Bishop von Rummel corresponded indefatigably, we are told, but the fragments that have survived are few in number and more often than not trivial.

It would be good to know whether, as the Jesuits suspected, Joseph's first tutor and life-long friend introduced into his instruction rationalist or Jansenist ideas foreign to Habsburg family piety. The author raises the question, but the evidence does not provide an answer. The same is true regarding Rummel's relationships with other powerful figures at the court. He had unusually free access to Joseph, but, except for a few relatively unimportant occasions, it is impossible to see how he used his influence. When Joseph sent his armies into the Papal States in 1708, Rummel appears to have stood by quietly in spite of insistent papal demands to put pressure on the emperor. As bishop of Vienna, Rummel apparently was content to do no more than function as his office demanded. His ten-year tenure produced no significant changes, no challenge to the close supervision the Austrian administration exercised over ecclesiastical affairs.

This book adds some useful fragments to our image of the Viennese court in the reigns of Leopold I and Joseph I. Of Bishop von Rummel himself we get only a dim picture of a diligent, compassionate prelate who was loyal to his patrons but not particularly effective in furthering their interests. The study is quite narrowly focused: there are virtually no references to major recent studies of the Habsburg monarchy by French or English scholars.

JOHN P. SPIELMAN
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JOHN W. BOYER. *Political Radicalism in Late Imperial Vienna: Origins of the Christian Social Movement, 1848–1897*. Chicago: University of Chicago Press. 1981. Pp. xvi, 577. \$35.00.

In this study John W. Boyer asserts that the Christian Social movement in a real sense was the bastard of Austrian liberalism. By 1897 it had so reaffirmed the class consciousness and the internal social homogeneity of the Austrian *Bürgertum* that it was sounding the knell of the inept and unlucky liberal politicians and preparing for the mortal struggle with social democracy. To prove his thesis the author points to liberal policies of the 1860s and 1870s that were of crucial importance to Viennese

politics: anticlericalism, devotion to the ideal of the autonomy of Austrian cities, and a gradual political detente with the state's bureaucracy. Their espousal of equality before the law and of decent educational opportunities did not save them from the challenge posed by increasing social tensions within the *Bürgerturn*, though they were never the mere representatives of great wealth and demonstrated a degree of proficiency in party organization and vote getting.

By the 1880s masters and journeymen in the crafts were economically hard-pressed, the lower clergy was in revolt against a quasi-Josephist episcopacy that seemed to do nothing to recapture the allegiance of the faithful, the university graduates were appalled by diminishing vocational opportunities, and the lesser bureaucrats were stymied by lack of chance to advance. A notable influx of Jews from Galicia and Bukovina provided the scapegoat that the disaffected required, and a motley group of agitators arose to preach economic and sometimes racial antisemitism. One of the less violent of these was Karl Lueger, who as a democrat, entered Vienna's city council in 1875 and only gradually realized that Jew-baiting could be politically profitable. By 1888 he was consorting with Karl von Vogelsang, editor of the *Vaterland*, and with Alois Liechtenstein, whose princely rank did not deter him from appeals to the street, and at the same time trying to restrain the excesses of extreme racialists such as Ernst Vergani and Ernst Schneider.

The Christian Social party over which he presided had unspectacular success in the local elections of 1891, but by 1897 a reluctant Franz Josef finally allowed Lueger the post of burgomaster in Vienna. The Vatican had refused to condemn him outright, Jewish voters were abstaining because of liberal indifference, the highest imperial advisers assumed that responsibility would tame or destroy him, and even the businessmen and professionals who had invested in rental property were turning to him as a savior.

Boyer's analysis is masterful in terms of research, exposition, and organization. His use of available economic data is judicious, and his sense of the social structure of late nineteenth-century Vienna is formidable. Vogelsang, Albert Gessmann, and Robert Pattai receive discerning appraisals, and much is gained by appropriate references to the fate of liberalism and antisemitism in the German empire. Lueger emerges as a consummate politician who was wary of doctrinaire clericalism, eager to reconstruct a consensus of the disparate elements within the *Bürgerturn*, and astute in his plans to remedy Vienna's financial plight through profitable and efficient operation of the utilities and transport facilities. If one can quibble, it would be with the author's conclusion that some regarded Lueger,

erroneously, as a dictatorial figure in party circles. In the reams that have been written on this figure, such verdicts may well appear, but his reliance upon Gessmann and his courting of German nationalists disillusioned with or banished by Georg von Schönerer were hardly unknown in the 1890s.

It is good to know that Boyer promises a concluding volume that will end with the year 1920. He is already alluding to the superior organizational and propagandistic skills of the Social Democrats. The stage is set for the tragedies of the interwar period.

WILLIAM A. JENKS

Washington and Lee University

KARL M. BROUSEK. *Wien und seine Tschechen: Integration und Assimilation einer Minderheit im 20. Jahrhundert.* (Schriftenreihe des Österreichischen Ost- und Südosteuropa-Instituts, Reihe Karlheinz Mack, number 7.) Vienna: Verlag für Geschichte und Politik. 1980. Pp. 148. S 198.

In this revised version of a Vienna dissertation, Karl M. Brousek addresses an important but often neglected aspect of the troubled relations of Czechs and Germans in the last two centuries. The development of ethnic and national identities in the two groups, contention for political and economic power, and assimilation from one group to the other took place not only in the Bohemian crown lands but also in the Habsburg imperial capital itself. In 1900 immigrants from the Bohemian lands or their children may have comprised as much as one-quarter of Vienna's population, and more than 6 percent of the citizens (103,000) reported Czech or Slovak as their language of everyday use. In 1971, however, only 8,110, or 0.5 percent, of the city's residents reported Czech as their language. Following up Monika Glettler's masterful study, *Die Wiener Tschechen um 1900* (1972), Brousek describes the political and social predicament of the Viennese Czechs after the dissolution of the Habsburg monarchy. A member of the Czech minority himself, Brousek takes up the cause of preserving a distinct Czech community in the twentieth century. He argues that persistent political and social divisions among the Czechs, the failure to establish a substantial propertied and educated upper middle class in the Czech community, and, above all, the continuing hostility of Austrian officials toward Czech-language education and minority representation have caused the far-reaching assimilation of Viennese Czechs with the German majority.

Those hoping for a worthy sequel to Glettler's work will be disappointed. Brousek proposes to explain the processes of integration and assimilation that have affected the Viennese Czechs since World War I, but he succeeds best in presenting a clear but

brief description of the Czechs' voluntary associations, schools, political efforts, and their experience at the hands of officials of the two Austrian republics and the Nazi regime. There is little systematic analysis in the body of the book; how and why assimilation has occurred is examined explicitly only in the conclusion, where the discussion comes almost as an afterthought rather than as the summation the author intended. Brousek's distinctions between spontaneous and forced assimilation and between reflective and unreflective assimilative processes may be useful, but to substantiate his arguments he needs a broader range of evidence to show concretely how Viennese Czechs adopted the dominant habits and values of their environment and changed their individual and group identities.

Brousek needs to examine much more carefully the character and development of social and economic interaction between the Czechs and the German majority in everyday life if he is to substantiate his assertion of the crucial role of political pressures in the assimilation of the Czech minority. Social or economic distinctions and boundaries between ethnic groups can help preserve group identities despite substantial acculturation. Glettler gave considerable attention to the Czechs' social structure and to their interaction with the rest of the population, but Brousek offers only a brief sketch of the social composition of the Czech minority and of Czech interaction with Germans. Brousek's book leaves much unfinished business, but the experience of Vienna's Czechs in the twentieth century is sufficiently important to understanding the social history of Austria as well as ethnic group relations throughout Central Europe that one hopes Brousek or others will continue the inquiry.

GARY B. COHEN

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IAIN FENLON. *Music and Patronage in Sixteenth-Century Mantua*. Volume 1. (Cambridge Studies in Music.) New York: Cambridge University Press. 1980. Pp. xi, 233. \$57.50.

This book is an important example of the growing interest in political and social history among musicologists. Led by such luminaries as Nino Pirrotta, Howard Mayer Brown, and Charles Rosen, these scholars are suspicious of the historicist assumption that musical styles, like ideas, flowed one from another in an independent progression. As Iain Fenlon puts it, "the function of music is a determinant of form and style, and when function changes new forms and styles arise while old ones tend to be modified and die out" (p. 6). Fenlon in fact keeps music itself secondary to consideration of its context in his study of patronage; he recognizes that social

and musical analysis ought not to be mixed. Historians will therefore find a kindred spirit here, and his ideas bear significantly upon a number of historical matters.

The study concerns patronage of music by the Gonzaga family in Mantua between about 1525 and 1598; it focuses primarily upon Cardinal Ercole and Dukes Guglielmo and Vincenzo. Beginning with an economical sketch of Mantuan society and economy, it maintains a broad sense of historical context without indulging in the clichés so common in cultural history. Fenlon looks at music within the court as a whole, discussing how performance patterns derived from the framework of courtly and clerical activities; he relates tendencies in music to those in architecture and painting. Historians will indeed find the book useful not just for information on musical life.

Fenlon sees patronage chiefly in political terms, as the use of music to undergird "despotic mythology" and assist in dynastic rivalries. Showing a sophisticated sense of the political dynamics of the period, he argues that music always served a purpose for the princes, even if the latter were quite serious as amateurs. He picks out particularly interesting political implications in references to music in frescoes, where music and harmony served as "a political metaphor." Like an increasing number of music historians, Fenlon sees only a limited influence of humanist ideas in such iconography. The point was usually less intellectual and more specific: to portray the glories of the prince.

The book suggests that dynastic ceremony grew in scale and significance during the sixteenth century, in musical terms culminating in the new genres of opera. If the point is incomplete here—clearer criteria for measurement are needed—it indicates an important dimension in the expansion of state culture around princely patronage. What emerges most fully is a sharp sense of how personal the style of patronage became under each prince, how both the political and musical aspects could change from one reign to another. On another plane, Fenlon shows the interaction between Mantua and both Florence and Paris, displaying the political and artistic aspects of dynastic competition.

Historians of early church reform will be especially interested in the discussion of how Cardinal Ercole and Duke Guglielmo applied the directives of the Council of Trent to their own purposes. As Fenlon puts it, "Ercole's patronage represents a local and early attempt at an integrated approach to the arts in the service of a reforming church" (p. 76). Discussion of the influence of Guglielmo on the sacred music written under his patronage is particularly important, as it represents one of the few efforts to date to evaluate the purely musical effects of patronage.

Indeed, the book is important for the care with which it analyzes patronage, a subject that in music history has usually been pedantic. Fenlon shows how patronage guided sacred music as much as secular, since erudition stood among the princely virtues. He also offers some sense of musicians' careers within the context of patronage, perhaps one of the least understood fields in music history. The volume has extensive extracts from archival sources and useful illustrations.

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RICHARD A. GOLDTHWAITE. *The Building of Renaissance Florence: An Economic and Social History*. Baltimore: Johns Hopkins University Press. 1981. Pp. xviii, 459. \$27.50.

Of all the industries of late medieval and early modern Europe, construction is possibly the one that economic historians have neglected most. Richard A. Goldthwaite aims at partially filling a serious gap by focusing on the unprecedented burst of building activity Florence experienced from the late fourteenth to the early sixteenth century. Part 1 of this truly impressive piece of scholarship is devoted to an analysis of the demand for building. Its roots are identified not only in the prosperity the city had achieved by the end of the fourteenth century as a textile, commercial, and financial center but also in the diffusion of wealth among a relatively large number of its citizens, in their changing attitude toward conspicuous consumption, and, lastly, in a newly felt need among the affluent to live in more spacious and more harmonious quarters.

Part 1 looks at Florence from the vantage point of the patrons who lavished money on palaces and churches; part 2 explores the supply side of the industry—construction yards, kilns, and quarries, but also the world of craftsmen, foremen, and architects. In two of his finest and most compelling chapters, Goldthwaite examines the structure and the role of the building crafts guild and the wages and living standards of the labor force. He convincingly shows that by the fourteenth century the guild had lost virtually all of its regulatory powers over the industry and that it had ceased to function as a welfare agency on behalf of its members. As for wages, their remarkable long-term stability stands in sharp contrast to the fifteenth-century downswing and the sixteenth-century upswing in food prices, with the result that a long period of high living standards was followed by one of increasing hardship for the construction workers.

In a concluding chapter Goldthwaite addresses the crucial and controversial question of what impact the construction "boom" that changed the face

of Florence had on the economy of the city as a whole. He persuasively argues that the partial reallocation of resources away from manufacturing and trade need not be written off as a total loss: besides sustaining the level of employment in decade after decade, the building industry of the Renaissance induced an astonishing demand for decorative objects, and the latter, in turn, resulted in the proliferation of artistic crafts that ultimately upgraded the quality of the labor force and left a precious legacy of skills and knowledge from which the city's economy has benefited down to the present. No less stimulating and refreshing is Goldthwaite's argument that, insofar as the Florentine artists and artisans successfully responded to the growing demand for luxuries, they spared their city the serious balance-of-payments problems that its economy would have been faced with had that demand been met with imports from abroad.

To my knowledge, no other study of the construction industry in the medieval and early modern periods can quite compare with Goldthwaite's exhaustive research, comprehensive approach, and penetrating analysis. His book stands as a model that I hope will inspire similar studies on the building industry in other cities of pre-industrial Europe.

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RICHARD C. TREXLER. *Public Life in Renaissance Florence*. (Studies in Social Discontinuity.) New York: Academic Press. 1980. Pp. xxvi, 591. \$45.00.

SAMUEL KLINE COHN, JR. *The Laboring Classes in Renaissance Florence*. (Studies in Social Discontinuity.) New York: Academic Press. 1980. Pp. xii, 296. \$29.50.

The two books reviewed here urge readers to look beyond standard approaches to Renaissance history in order to establish the linkages between high culture, political culture, and social change.

Richard C. Trexler sets out to examine the role of ritual behavior in maintaining and transforming Florentine society. He suggests, "Only if we understand how the individual Florentine citizen worked through forms, and how he experienced such experiments can we finally understand the significance of the whole commune at work" (p. 129). To establish the relationship between ritual and the forms and values of the Renaissance city, Trexler examines how different social groups and individuals related to each other and to internal and external social forces. As he traces the decline of public ceremony during the Renaissance, he also details the loss by most Florentines of immediate control over their destinies to larger economic and political units.

After a description of social and political frameworks one reaches the most rewarding sections of the book, which describe and analyze three significant stimuli—the need to celebrate communal life during stable eras, the need to impress important foreigners, and the need to protect the social and political fabric of the city during threats—and the various communal responses to them. Around this analysis Trexler describes the impact of change and revolution as the republican regime gave way to the hegemony of the Medici.

The Laboring Classes in Renaissance Florence is based on many of the same manuscript collections utilized for *Public Life in Renaissance Florence*, but Samuel Kline Cohn, Jr., has been more imaginative in transforming these into quantitative data, which are brought to bear on the significantly changing political role of Florentine workers.

The first part of Cohn's book is standard demographic history, including analyses of immigration, social networks, and population distributions across neighborhoods. Centering on marital relationships, Cohn describes the changing social patterns of the two significant social classes of the Renaissance city, the patriciate and the *popolo minuto*. From the late fourteenth to the fifteenth century the elite moved from a neighborhood to a cosmopolitan orientation, while during the same years the social networks of the workers turned inward. As such, the Florence of Lorenzo was a city more clearly divided along class lines than in previous eras.

Cohn then focuses on the social and political consequences of these changes. The crowds of late fourteenth-century riots and insurrections are examined from the perspectives of leadership, composition, and ideology. Following Tilly and Rudé, Cohn discovers strikingly modern patterns, especially in the revolt of the Ciompi. But this is the high point of the class struggle. Cohn relates the declining impact of popular protest to the shift toward neighborhoodness in the workers' social milieu, and to the concept of uneven development. Economic and work modes of fourteenth-century Florence approximated those of late eighteenth-century Paris, even though the nature of the state was radically different in the two cases. As a class, then, the *popolo minuto* became integrated into the city as a geographical entity, enabling both citywide organization and insurrection. But by the fifteenth century the patriciate was in control of enhanced state apparatus, and successful insurrections were doomed to failure. In this era expressions of class conflict hardly exist; even the minor examples Cohn cites might in another's hands be treated as random acts of criminality.

These changes were the result of the parochialization of the Florentine working class. This in turn

was largely the result of changes in the labor supply—in the form of an influx of migrants, including many non-Italians—and of the failure of the Ciompi revolt, which broke the back of working-class militancy and resistance in much the same way that the failed General Strike of 1926 gave way to an era of labor quiescence in Britain.

As a revisionist Cohn's major targets are all those who interpret Renaissance cultural or political elites without understanding the impact of the *popolo minuto* on either, and the work of Gene Brucker is especially singled out in this regard. Although many historians will find fault with the way the data are treated, the value of both books transcends Renaissance historiography. All historians working in European urban history might consider the emphasis Trexler and Cohn place on studying cities through formal behavioral structures and the merger of social and political-cultural history.

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LAURO MARTINES. *Power and Imagination: City-States in Renaissance Italy*. New York: Alfred A. Knopf. 1979. Pp. ix, 368. \$15.95.

Rich in detail and vigorous in argument, Lauro Martines's history of Italy from the eleventh to the sixteenth centuries is a sweeping synthesis of research on the Renaissance. Half the book is devoted to "power," that is, to tracing the development of the Italian city-state from a loosely organized, sometimes anarchic commune to a signorial and territorial state. Florence, of course, is given its due, but Martines also ranges across central and northern Italy, bringing in examples from Perugia, Orvieto, Cremona, Bologna, and Genoa to point up the unity of communal political history. In fact, one of the virtues of Martines's account is that Milan is given prominence as the city-state that best exemplified the tensions and impetus of political authority.

Covering so much ground and so many years, the book threatens to overwhelm the reader with a welter of illustrations, and Martines occasionally even resorts to lists to convey important and complex developments. Moreover, the march of active verbs can be tiring. Successive sentences, for example, parade abstract qualities that "awakened," "sifted in," "swelled over," and "burst into" (pp. 16–17). Martines, however, is always in control of his material, and his headlong pace is slowed by insightful essays that concentrate on narrow topics, such as the urban values of the Anonimo Genovese (pp. 87–93), the political attitudes of Brunetto Latini (pp. 115–23), and the failure of the Ambrosian republic (pp. 140–48).

Martines's discussion of "imagination"—"the articulated, formal, refined, or idealizing consciousness of those who speak for the powerful" (p. ix)—relates painting, sculpture, architecture, and literature to the ideals and interests of Italy's ruling elites. Such an unabashedly materialist view of high culture risks the charge of reductionism, but Martines is less interested in culture *per se* than in the ways in which culture expresses the values and needs of dominant social groups. He does not deny the transcendent worth of art but, rather, aspires to correct an excessively elevated view of Renaissance culture. He resists the powerful temptation to put politics in one compartment, art in another. Thus Giotto's naturalism stemmed in part from his immersion in a mercantile commune (p. 250). Whatever its contribution to civilization, humanism in the Renaissance was "a program for ruling classes" (pp. 191–217). The "hyperorganized rationalism" of Quattrocento painting expressed the domineering and arrogant confidence of the elite (p. 259). The troubled idealism of Castiglione's *The Courtier* reflected the fractured social identity of the upper classes (pp. 330–31).

Perhaps inevitably, Martines's approach leads occasionally to merely confusing or trivial conclusions. Mantegna's *San Zeno Altarpiece*, then, "somehow joined—but did not transcend—conflicting social identities" by combining controlled organization with luxurious ostentation (p. 268). And Martines makes the wisely tentative assertion that, in the Cinquecento, Italy's "social and economic difficulties, as recorded along the track of patronage, were summed up, arguably, in Torquato Tasso's bouts of insanity" (p. 303). But all in all, this is a powerful and imaginative work, as deliberately ambitious, bustling, and provocative as the subject it surveys.

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ECO O. G. HAITSMAN MULIER. *The Myth of Venice and Dutch Republican Thought in the Seventeenth Century*. Translated by GERARD T. MORAN. (Speculum Historiale, number 11.) Assen: Van Gorcum; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. xii, 237. \$20.00.

This book studies various related subjects. It starts with a discussion of the "myth of Venice" as it appears in sixteenth-century Italian authors and then considers its influence on political thought in France and England, concentrating on Seyssel, Bodin, Milton, and Harrington. Although Eco O. G. Haitsma Mulier has carefully read and pondered all these authors, he of course relies heavily on the

large number of excellent studies already available. His main purpose is therefore to sketch the background against which the Dutch development—the author's more specific object of research—has to be seen.

In the Dutch context, Haitsma Mulier studies mainly Graswinckel, the brothers De la Court, and Spinoza. Graswinckel published in 1634 a volume in Latin in which he tried to refute the *Squitinio della libertà veneta* (1612), whose anonymous author had attacked the idea of Venice's immemorial autonomy. Haitsma Mulier describes how Graswinckel, making use of secret archive material possibly provided by the Venetian dignitary Dominico Molino, attempted to prove the *Squitinio* wrong. As Haitsma Mulier says nothing about the influence of Graswinckel's book on the Netherlands or abroad and as the book itself contains only little political theory proper, this long chapter (pp. 77–119) can best be read as a separate study of interest to the student of Venetian history rather than of Dutch political thought.

The rest of the book (pp. 120–208) is devoted to the analysis of Italian influences on the writings of the brothers De la Court and Spinoza. It is convincingly proved that Machiavelli loomed large among the authors whom these theorists studied. In the second place, it is indicated with more precision than ever before that they used specifically Venetian political institutions as models for their rather theoretical proposals to reorganize Dutch institutions. This close and detailed analysis is helpful. Although we may doubt whether, in a more comprehensive survey of the thought of these authors, the Venetian model would deserve as much emphasis as it gets here, Haitsma Mulier serves his readers excellently by providing this interesting approach. With the De la Courts and Spinoza, however, we are fairly far from the Venetian "myth." There is little that is mythical in their way of studying Venice's institutions and devising Dutch variations on the composition of its councils and its ballot system.

This is a good and learned book but one strictly for the initiated. It is a Dutch Ph.D. thesis well translated into English but unfortunately not adapted to the non-Dutch reader. The original Dutch reader was helped by useful information about Venetian institutions and Venetian political writers. The non-Dutch reader of the English version does not receive such help in connection with Dutch institutions or Dutch authors. Moreover, Haitsma Mulier has overdocumented and overannotated his thesis to such an extent as to make it unnecessarily forbidding. The specialist in seventeenth-century political thought, however, will warmly welcome it.

E. H. KOSSMAN
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EDMUND E. JACOBITTI. *Revolutionary Humanism and Historicism in Modern Italy*. New Haven: Yale University Press. 1981. Pp. xii, 248. \$18.00.

The title of this book by Edmund E. Jacobitti promises a good deal more than it achieves. It is essentially a study of Benedetto Croce from his earliest writings until the First World War. An initial chapter considers the contribution of "revolutionary" humanists such as Giambattista Vico, Vincenzo Cuoco, Bertrando Spaventa, and Francesco De Sanctis to the problems of humanism and culture that dominated Croce's thought. His accomplishment is viewed as a consequence of his search for a secular faith appropriate to mankind's creative responsibility for its own destiny. Croce's certainty and confidence is credited to his rejection of positivism and Marxism, his view of history as art, and the ethical praxis at the heart of all his criticism and philosophy. Croce's celebration of man's divinity denied authenticity to all social philosophies centered on man's limits and the constraints of history.

Once Croce's system was in place he was prepared to launch the influential review *La critica* (1902-43) to wage unending combat against positivism and determinism; and more importantly to develop a profound culture equal to the historical tasks confronting Italy and modern man. Early in the life of the journal, Jacobitti very interestingly notes, Croce unstintingly praised Giosuè Carducci's "Hymn to Satan." Croce wrote, "The *Inno a satana* is the celebration of human history, of the force of reason and a healthy sense of life: Satan, symbol of nature in antiquity, reappears as the sorceress, wizard, the alchemist of the Middle Ages, in so far as they planted the seeds of truth and life for the future." Unfortunately, Jacobitti does not adequately reflect on the historical and anthropological insights that permitted Croce's interpretation but is content with another comment on the celebration of man as the message of the text.

Croce's commitment to what man might accomplish did not permit him to tolerate in any fashion those he held to be in error. But he was most indulgent toward *i fedeli*, and Jacobitti perceptively discusses this generosity in Croce's relations with Georges Sorel. His supporters, however, did not free Croce from the need for a final assault on Hegel, Marx, and Antonio Labriola. Jacobitti skillfully illuminates the dimensions of this mature endeavor by examining Croce's "absolute historicism."

In a valuable last chapter Jacobitti concludes that by the time of the war Croce was a "prophet who failed." Young writers now saw his system as belonging to him alone, not a faith to which others could give their assent. But nothing in Jacobitti's analysis

prepares his readers for this denouement. He elegantly paraphrases Croce's views on ethics, on Hegel, on Marx, on history but never in the body of his text considers the limits of Croce's understanding. Content to use as the binding threads of his exegesis terms such as religion, faith, and spirit, Jacobitti does not ask why Croce was dependent on these concepts nor question their adequacy in constructing a lasting secular humanism.

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GIULIO CIANFEROTTI. *Il pensiero di V. E. Orlando e la giurispubblicistica italiana fra ottocento e novecento*. (Quaderni di "Studi Senesi," number 48.) Milan: Dott. A. Giuffrè Editore. 1980. Pp. xii, 465. L. 20,000.

Vittorio Emanuele Orlando has the misfortune to be remembered principally for his failures at the Paris Peace Conference in 1919 and less for his successes in leading Italy during its last year of war. Even less is he known in the English-speaking world for his contribution to jurisprudence. Other Italian writers have done him justice for his writings on public law and administration; it is the extraordinary merit of Giulio Cianferotti's work to place Orlando's writings in the context of a vast jurisprudential literature that both shaped Orlando and to which he contributed. To read Cianferotti is to receive an education on a major part of modern European jurisprudence. He combines a dense but lucid text with voluminous explanatory and reference notes that not only double the length of his work but also constitute what is virtually a sub-volume.

Inevitably in any work on modern jurisprudence, German authors figure largely and sometimes predominate; certainly they influenced Orlando's early thought. But without renouncing the influence of Friedrich Karl von Savigny, Johann Kaspar Bluntschli, Rudolf von Gneist, Paul Laband, and others, Orlando eventually freed himself from what might have become a stifling dependence, and evolved from them, as he did from Spencerian positivism, to a position autonomous and original: the reconciliation of parliamentary government with administrative rectitude through the construction of a juridical order that makes *Rechtsstaat* and parliamentary government identical. The *Rechtsstaat* remains central, but it is modified by the distinction between political—that is, collective—liberties and juridical liberties, which are civil or individual. The German *Rechtsstaat* emphasized civil liberties; French doctrinaire liberalism emphasized political liberties; the Orlandian purpose was to reconcile the two into an Italian *Rechtsstaat* that would constitute

the culmination of national unification in the liberal state. This endeavor was founded on a strict "logical-juridical" formalism that reduces all historical-political, philosophical, sociological, and psychological considerations to a secondary plane so that the content of the *Rechtsstaat* is solely one of law, and its only reality is a juridical one.

In the first half of the book, the fruits of nearly twenty-five years of Orlando's efforts are delineated and analyzed by Cianferotti with meticulous and detailed references to works by Italian and foreign writers. Not so clearly expounded is the nexus between Orlando's thought and the consequences for the functions of the state of the long depression that began in the 1870s. True, Cianferotti mentions that this depression occasioned new state political-administrative functions, which occasioned in turn corresponding juridical reflections. But the thesis, although plausible, lacks elaboration.

The second half of the volume is devoted to writings on public law during the Fascist and post-Fascist periods, particularly as these writings relate to Orlando's thought. Cianferotti deals fairly with the arguments of Alfredo Rocco and others who contended that Fascist thought on the state constituted a resolution of the logical contradiction between Orlando's tenet that the juridical state, not the people, is sovereign and Orlando's definition of this state in liberal-democratic terms. But not all Fascist writers were ready to accept Rocco's thesis of a continuity between Fascist doctrine and the Orlandian national juridical school. Philosophers of law, corporatists, and champions of the comparative historical method criticized it as an abstraction divorced from reality, allegedly separating totally the state from society, form from substance, theory from history. But this criticism, at times acute and appropriate, did not lead to the creation of any new viable approaches, as is evident from the inconclusive literature of the 1920s and 1930s. The second postwar period reveals first an attempt to vindicate the Orlandian logical-juridical approach and then a crisis in this approach, foreshadowed by the affirmation of a Weberian sociology of law. But for Cianferotti the greatest crisis of juridical formalism arises from the crisis now faced by the formalism of state power itself, dissolving as it is in a variety of pluralisms. For this crisis he finds neither solution nor adequate understanding in Weberian sociology or in Marxism.

One need not accept all of Cianferotti's judgments, either on Orlando and his school or on its critics, to appreciate Cianferotti's work as a masterful synthesis of its subject.

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RICHARD CLOGG, editor. *Balkan Society in the Age of Greek Independence*. Totowa, N.J.: Barnes and Noble. 1981. Pp. vi, 253. \$29.00.

Delivered at a sesquicentennial symposium on the Battle of Navarino (October 20, 1827), five of the papers in this book deal with Greek society between 1780 and 1830, four with the contemporary neighboring Balkan societies, and one—by C. M. Woodhouse—with the Battle of Navarino itself, "the last great battle of the age of sail" (Richard Clogg, p. vi).

In "Greek Society on the Eve of Independence," Yannis Yannouloupoulos focuses on the Ottoman economy. Lacking a free labor market, that economy was characterized by the presence of "zones of money economy" and of a trading sector articulated less in the realities of local production than in the demands of international commerce and the commands of the redistributive but generally antimercantilist economy. In the Ionian Islands, on the other hand, there was, according to George Yannouloupoulos, a different mode of production, defined by the concentration of land and political authority (hereditary membership in the insular assemblies) in the hands of a nobility. Employing hired labor or leasing out parcels of land to peasants for a designated number of years, the nobility lacked the right to modify the terms of the leases. Nor were they free to engage directly in commerce. Created mainly during the long Venetian occupation, the above mode of production came under attack after the French occupation of the islands in 1797 through the challenge to the political authority of the nobility, source of their economic power.

In "The Greek Intelligentsia," Peter Mackridge notes the diffusion among Greeks during the second half of the eighteenth century of the old quarrel between ancients and moderns, supporters of the superiority of ancient learning versus advocates of modern science. Richard Clogg throws more light on this subject by showing that merchants and intellectuals with a modern orientation in some spheres of thought continued to hold archaic beliefs in others, including prophecies of salvation by the act of a "fair-headed race" (presumably the Russians). Without the collapse of their commercial profits between 1815 and 1820, he suggests, Greek merchants and shipowners may have been even more reluctant to rise against Ottoman rule. According to Viron Karidis, Greek merchants competed to advantage with Western European merchants in the grain trade of Odessa through the constitution of the Greek diaspora as a network of information bound by religion, language, community, and kinship.

The remaining papers provide a comparative dimension through the descriptions of Serbia (by

Stevan Pavlowitch) as a federation of village republics enticed into rebellion to arrest the spread of sharecropping practices; Bulgaria (by R. J. Crampton) as a country of varied systems of land tenure and developing textile manufactures; Montenegro (by Alan Ferguson) as a land of tribesmen and vendetta; and the Rumanian provinces of Wallachia and Moldavia (by Denis Deletant) as lands of venal officials (boyars) with claims to peasant labor and of monastic landowners, under the intellectual, political, and commercial domination of Greeks, subjected to Ottoman tributes, increasingly open to French cultural (but not then revolutionary) influences. What is lacking in the volume is a conclusion or synthesis, an attempt to relate the papers.

TRAIAN STOIANOVICH
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JOAN FULTZ KONTOS. *Red Cross, Black Eagle: A Biography of Albania's American School*. (East European Monographs, number 75.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. xv, 216. \$17.00.

Owing principally to widespread sympathy in Albania for President Wilson, to the enormous prestige of American-educated Bishop Fan S. Noli, and to the influential role in Albanian affairs of the Pan-Albanian Federation of America (VATRA), Albania in the 1920s was fertile soil for the growth of American influence. The American presence was evident in such institutions as the Phineas Kennedy School for Girls in Korçë, the American School of Agriculture in Kavajë, the antimalaria efforts of the Rockefeller Foundation, and the Albanian Vocational School (often referred to as the American Technical School) in Tiranë.

This fine book by Joan Fultz Kontos tells the story of the Albanian Vocational School (AVS) from the time of its establishment in 1921 until its nationalization by King Zog's regime in 1933. The book is dedicated to the two primary figures of the school, Charles A. Hollingshead (1867–1954) and the author's father, Harry T. Fultz (1888–1980), who directed AVS from 1922 to 1933. Both men were exceptionally able and devoted educators.

AVS operated under the auspices of the American Junior Red Cross. It was funded mostly by the Junior Red Cross with the balance of funds coming from the Albanian government and from income generated by a variety of commercial enterprises of the school. These included a printing shop, an electric plant, an ice plant, and a farm.

Writing in a restrained, scholarly, and dignified style, Kontos gives a detailed and vivid account of

the twelve-year life span of AVS. She makes effective use of documentary materials to tell about the growth of the school from an initial enrollment of 32 students in 1921 to over 500 in 1933 and about the teaching program of AVS which included not only vocational training but also instruction in mathematics and the social and physical sciences, plus a rich roster of extracurricular activities, such as dramatics, publication of a school newspaper, *Laboremus* ("Let Us Work"), and sports, including baseball. But beyond these, the students at AVS learned occupational, social, and human skills and values of inestimable worth, not the least of which was "the dignity of work," especially manual labor.

The story of AVS is told against the background of an evolving and fluid Albanian society. As a result the book gains greatly in scope, vitality, and meaning.

Among the 156 alumni of the school are the names of Mahmoud Tsungu, Hamdi Kerçiku, Mustafa Gjinishi, and Mehmet Shehu, Albania's prime minister since 1954.

This well-written book enriches the scholarship on Albania. It increases our understanding of an American initiative to contribute to the progress and enlightenment of a very backward Albania of half a century ago through the development of education. The book shows year by year the trials and frustrations, but also the rich rewards of a "pioneer program of technical assistance" (p. xiii), as implemented by AVS.

The book has a good foreword by Nicholas Pano. It has, in addition, over a dozen pages of illustrations, notes, bibliography, and index.

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ȘTEFAN ANDREESCU. *Restitutio Daciae: Relațiile politice dintre Țara Românească, Moldova și Transilvania în Răstimpul 1526–1593* [The Return of Dacia: Political Relations among Wallachia, Moldavia, and Transylvania, 1526–93]. Bucharest: Editura Albatros. 1980. Pp. 235. 5.50 L.

Although traditional Rumanian historiography emphasizes the role and importance of Stephen the Great and Michael the Brave in the early development of the Rumanian state, recently scholars have begun to examine the events and personalities that shaped the history of Moldavia, Wallachia, and Transylvania in the years between the reigns of these leaders. This recent monograph is a worthy addition to that body of literature.

Andrescu asserts that the temporary unification of the three Rumanian provinces by Michael in 1599–1600 was not an isolated event; instead, it represented the culmination of a historical process

that began in 1526 at the Battle of Mohacs. That engagement led to a vacuum in leadership and a rearrangement of political loyalties in these areas critical for the emergence of an early national identity and unity among Rumanians. Throughout the remainder of the century the political and cultural ties binding the regions grew stronger. Without an understanding of this movement, the author contends, the events of 1599–1600 cannot be comprehended fully.

Using a strong mixture of primary and secondary sources, Andreescu analyzes the three regions separately in order to show the centripetal forces that brought them into alignment by the end of the sixteenth century. Naturally the most controversial aspect of this analysis concerns Transylvania. Andreescu holds that its unique geographical position in the struggle between Austria and the Ottoman empire brought the area increasingly into the orbit of the other two Rumanian provinces rather than those of the principal belligerents. Although at times he overestimates the ties to Moldavia and Wallachia, the development of this argument, which underscores his general thesis, adds to the work's merit.

The book is not, however, without certain flaws. The first and most obvious drawback is careless editing; not only are numerous words misspelled but others are also left out of the text entirely. Second, references to less than renowned historical figures can be distracting at times; brief biographical footnotes would be helpful to the general reader not fully versed in the events of the sixteenth century. Third, in dealing with the sensitive nature of the Transylvanian question the author is too selective in his use of supporting evidence. Finally, although the first chapter contains an excellent short survey of the historiography of the period, greater attention could be devoted to non-Rumanian sources without detracting from the overall thesis.

These criticisms notwithstanding, this is a valuable addition to a relatively underdeveloped area of Rumanian historical scholarship. As such, it should prove to be useful to scholars not only of Rumanian history but also of this period of Balkan history as well.

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ANDREI PIPPIDI. *Hommes et idées du sud-est européen à l'aube de l'âge moderne*. Bucharest: Editura Academiei Republicii Socialiste România or Éditions du C.N.R.S., Paris. 1980. Pp. xi, 372. 30 L.

Since the modern, critical phase began in Rumanian historiography with A. D. Xenopol nearly a century ago, two general lamentations may be found con-

cerning historical writing on the Rumanian Enlightenment and the eighteenth- and early nineteenth-century epoch of the so-called Phanariot regime: first, that world history (read, European history) as perceived and taught to the broad public continues to be almost exclusively Western history, ignoring the crucial role in the evolution of the modern state system played by the lands east of the Elbe; and second, that Phanariotism is a term necessarily coincident with retrogression, evil, and exploitation. A number of important Rumanian studies have appeared since 1945 aimed at revising both of these tendencies, and the present collection by Andrei Pippidi must certainly rank as one of the most important yet. In large measure it is the final installment of a triad of works begun by D. Popovici's *La littérature roumaine à l'époque des lumières* (1945) and expanded by Vlad Georgescu's brilliant *Ideile politice și iluminismul în principatele române, 1750–1831* (1972). Now in republishing fourteen of his most detailed articles (all but one of which appeared between 1968 and 1979 in Rumanian or French scholarly literature), Pippidi reinforces the integration of Rumanian and Western Enlightenment culture begun by Popovici; expands on the theoretical and general notions held by the Rumanian illuminati, which were first extensively assayed by Georgescu; and, by dwelling on the period from the sixteenth through the late eighteenth centuries, casts the origins of Rumanian "Iluminismul" backward in time and revises the image of the Phanariots further by offering the category of "pre-Phanariotism" to the Rumanian historical spectrum.

Here we have one of the most thorough analyses (complete with extensive explanatory footnotes), that we are to find of early modern travel literature about the lands of the Morlaques and Valaques, during the period when Westerners saw the Danubian areas as regions of the "Noble Savage." We proceed through studies of "The Danubian Lands and Lepanto" and "The Resurrection of Byzantium or Romanian Political Unity," which dwells on Michael the Brave, to "Relations of Raguse (Dubrovnik) With the Romanian Lands," which is followed by close studies of notables such as Constantin Brâncoveanu, Dimitrie Cantemir, and Nicolae Mavrocordat, whose library is closely catalogued. Although the section on Nicolae Soutzo (Șuțu) is almost obligatory in a work of this type and contains little that is surprising, the chapter on "Jean Caradja and His Friends in Geneva" will be largely new to Western students. The sum total is to display the gradual increase in economic, political, and intellectual contacts between the Danubian provinces and the West, all of which destroys the traditional notion that Westerners viewed their Phanariot counterparts as people *à part*.

Internally, Pippidi convinces us that "a new social class of functionaries in the state was formed at least twenty years earlier than the date fixed by tradition for the onset of the Phanariots" (p. 348) and that one of its distinctive traits was the enlargement of intellectual horizons. A twenty-one page index of names, ample illustrations, large fold-out genealogical charts, and the combined notes that constitute an exhaustive bibliography for early modern Rumanian and Ottoman history all add to the value of this important work of reference and inspiration.

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DRAGUTIN PAVLIČEVIĆ. *Narodi pokret, 1883., u Hrvatskoj* [The National Movement, 1883, in Croatia]. Summary in German. (Biblioteka Znanstvenih Radova.) Zagreb: Sveučilišna Naklada Liber or Sveučilište u Zagrebu—Institut za Hrvatsku Povijest, Zagreb. 1980. Pp. 394.

Sometime during the wee hours of August 7, 1883, Antal Dávid, the Magyar director of the joint Hungarian-Croatian finance service, replaced the coats of arms that hung on his offices in Zagreb, capital of the semi-autonomous Habsburg province of Croatia-Slavonia. Along with the usual inscription in Serbo-Croatian, the new arms bore an additional inscription in Magyar. This seemingly insignificant act sparked both street demonstrations in Zagreb and other Croatian cities and widespread peasant disorders that left 48 dead and many more wounded. It also ignited a political crisis that led to the resignation of the Croatian *ban* (governor), Ladislav Pejačević, a temporary suspension of the constitution in Croatia, and the beginning of the twenty-year, iron-handed governorship of Károly Khuen-Hederváry.

Sponsored by the Institute for Croatian History at Zagreb University, this book by Dragutin Pavličević is the first archivally based monograph in Serbo-Croatian on these events. The author more than fulfills the reader's expectations of high scholarship raised by the institute's sponsorship. His is a measured, reasonable, and exhaustively detailed account of the background, course, and resolution of the crisis, solidly based on archival sources, contemporary newspapers, and the memoir literature. Of particular value is his survey, village by village and town by town, of all the disorders. Pavličević supplements his account with three maps, two indexes, numerous tables, contemporary illustrations, a German summary, and an introductory historiographical essay. In the latter he assesses previous work on the subject in Serbo-Croatian as well as the research of the Hungarian historian, László Kátus; unfortunately, he does not mention the unpublished disser-

tation by Manuela Dobos, "The Croatian Peasant Uprising of 1883" (Columbia, 1974), which uses virtually the same sources as his work.

Pavličević demonstrates that the long-term causes of the Croatian crisis in 1883 were a complex amalgam of political and economic factors. Not surprisingly, however, he stresses the economic factors, particularly with regard to the peasant disorders: the deepening agrarian crisis that followed the depression of 1873, the tremendous increase in tax burden along with the concomitant harshening of collection methods, the decline of the *zadruga*, and, finally, the peasants' lack of economic alternatives in undeveloped Croatia all led to the rural outbreaks. According to Pavličević, far from being a strictly political, much less national, movement, the events of 1883 represent "a reflection of the socioeconomic and political crisis of Croatian society in the transitional era after the abolition of feudal relations" (p. 357). Nevertheless, he argues that the crisis also constitutes a political watershed, since for the first time the peasants identified themselves with Croatian nationalism and entered Croatian political life as an active force that the urban political parties could no longer afford to ignore.

Pavličević's emphasis on the importance of economic causes is convincing, even if one does not accept his Marxist framework. Surprisingly, though, he devotes relatively little space to the issue of the secretly divided *zadruga*, although Dobos has convincingly correlated the geographic extent of this phenomenon with precisely those areas where the peasant disorders occurred. Moreover, without further documentation, the author's depiction of the peasantry as a consciously Croatian, active force remains only a possible alternative to, and not a refutation of, Dobos's argument that the disorders were more a traditional peasant reaction to oppressive conditions.

These caveats aside, Pavličević's book is an excellent survey of the events of 1883 in Croatia, one to which no short review can do full justice.

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FRED H. EIDLIN. *The Logic of "Normalization": The Soviet Intervention in Czechoslovakia of 21 August 1968 and the Czechoslovak Response*. (East European Monographs, number 74.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1980. Pp. viii, 278. \$20.00.

This book by Fred H. Eidlin attempts to link a selective description of the first week of the Soviet invasion of Czechoslovakia in August 1968 with a conceptual and methodological tract on an assortment of political science precepts. The purpose of

the exercise is difficult to understand as Eidlin seems to put forth many more questions than his concepts allow him to answer. A proper conclusion is lacking; instead the preface is rephrased at the end of the volume.

An obstacle created by the author is that he looks at the Soviet intervention and the ensuing "normalization" from the point of view of the first postinvasion week to ten days. He fails to say that his statements are not valid for a longer period of time, thus confusing issues. He also overrates, in my opinion, the scope of power left to Dubček and company after their return from Moscow in the wee hours of August 27.

Eidlin has some strange things to say, which are not supported in any clear-cut way. According to him we should not overlook "all the evidence suggesting that the Soviets were, despite their deep anxieties about the situation in Czechoslovakia, at least partially convinced by the arguments of the Czechoslovak reformists that the reformist policies being pursued were necessary, and would lead to a more stable and viable regime which would be a faithful and reliable ally of the Soviet Union" (p. vii). What is the evidence for this assertion? Why then did Moscow not allow some of the reforms to continue under its control after the defeat of the Prague Spring?

At times Eidlin appears to want to breathe his own premises into historical developments. Thus he supposes the Soviets wanted to stage a rapid normalization within days of the invasion, and he then draws conclusions from their not doing things so quickly, without taking proper account of the possibility that they might not have been in such a rush (in some respects) in the first place.

Pseudoproblems are presented as genuine issues. For example, Eidlin's cardinal question (styled "A" on page 7) calls for an examination of why Moscow did not act more forcefully when it did have the power to do so. This is not logically inconsistent, because "unchecked sovereignty" obviously does not mean that the ultimate measure of force has to be employed at all times. Eidlin himself discusses this issue not much later. The *raison d'être* of his "cardinal" question thus becomes muddled.

The use of quotations and paraphrases from other people's works leaves something to be desired. Karl Popper is, for example, quoted in a way that permits an interpretation contrary to his argument (p. 7). The Simes-Valenta controversy is also presented inaccurately and without mention of the fact that Valenta accepted a part of Simes's argument in his book. This reviewer is also said to have been "puzzled" in his *From Dubček to Charter 77*, when in fact he only pointed to some developments that could not be satisfactorily explained from available evidence.

In all, Eidlin has not written a book on the logic of normalization. Not even the most starry-eyed Soviet politician would believe that Czechoslovakia could be "normalized" in a week or so. Instead, Eidlin has set out to argue that the Soviet intervention plan failed. The results of the book carry little surprise, mostly because no one has ever really doubted that certain things went wrong for the invaders. I cannot help feeling that a microanalysis of the events of that crucial week, which Eidlin describes in some detail, would have served the reader in better stead than the yawning heights of ratiocination.

VLADIMIR V. KUSIN

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LESZEK KAMIŃSKI. *Romantyzm a ideologia: Główne ugrupowania polityczne Drugiej Rzeczypospolitej wobec tradycji romantycznej* [Romanticism and Ideology: Principal Political Groups in the Second Republic in Relation to the Romantic Tradition]. (Biblioteka Towarzystwa Literackiego im. Adama Mickiewicza, number 12.) Wrocław: Ossolineum. 1980. Pp. 131. 35 Zł.

This is a brief, very specialized monograph on a very Polish topic. To review it adequately and to place it in a broader context of literature, it would be necessary first to write a historiographical essay on post-1945 scholarship dealing with Poland between the two world wars. Two notable articles may serve as a substitute for such an introduction: one, by Andrzej Garlicki, Tomasz Nałecz and Wiesław Włodyka, was published in *Przegląd Historyczny*, (vol. 3 [1978]); the other, by a writer hiding behind the cryptonym of "ZK," appeared in *Res publica*, (vol. 6 [1980]), and independent, that is, uncensored, review also published in Warsaw, and was reprinted in the *Paris Zeszyty historyczne* in its May 1981 issue.

In the Polish political mind, as any reader of Adam Bromke's *Poland's Politics: Idealism versus Romanticism* (1967) knows, literature and ethics, philosophy and religion, form an indissoluble combination. It is not surprising, then, that Leszek Kamiński's book examines the stand taken by inter-war Poland's main political groups on the question of romanticism. Was Polish romanticism, that nineteenth-century literary, philosophical, and political movement whose three most famous figures died before 1860, a live, controversial issue before the Second World War? Kamiński argues that it was. He examines select periodicals that in one way or another, sometimes informally, were connected with the Sanacja (the Piłsudski camp), the Endecja (the nationalist opposition headed by Dmowski), and "The Left" (where he places both socialist and Communist spokesmen), in order to define their respective views of romanticism. The author is not interested

in exploring the positions of individual critics; rather, he seeks to identify the stands taken by representative journals as a step toward constructing the "models" of the Sanacja, Endecja, and Left outlook.

The author admits that there are problems with his approach. His topic falls between literature and politics but does not cover both nor is limited to one. This means that some major periodicals are omitted because they were not tied to political parties even when, as in the case of *Wiadomości Literackie*, the most famous of all, they certainly had an identifiable political orientation. Nor are all major political currents, such as the peasant movement or the Christian Democrats, covered. Finally, the author is aware that the ostensibly literary and philosophical discourses on the darker pages of Mickiewicz or Słowacki, when presented in primarily political journals, often served, for example, to support or deny Piłsudski's claim to govern Poland. Was romanticism really the issue? For the historian, this book by a literary scholar offers an unusual but revealing insight into Polish politics. It is interesting to learn that the polemics over the romantic poets dealt with such basic and topical questions as the geographical identity of Poland and the nature of Polish nationality in the twentieth century.

ROMAN SZPORLUK
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A. G. MAN'KOV. *Ulozhenie 1649 goda: Kodeks feodal'nogo prava Rossii* [The Law Code of 1649: Codex of Russia's Feudal Law]. Leningrad: Nauka. 1980. Pp. 269. 2 r. 10 k.

The law code (*Ulozhenie*) of 1649 was compiled on the heels of a riot in Moscow in June 1648. A special commission of churchmen and secular notables employed Muscovite precedents and other sources to prepare a draft of the code that was subsequently confirmed at a special meeting of the Assembly of the Land late in 1648 and early 1649. As A. G. Man'kov notes, this code was different from all its Muscovite predecessors, not only in bulk and organization, but also in its confirmation of a stratified social order, what Man'kov calls "feudal society." Furthermore, the *Ulozhenie* remained important to imperial Russia inasmuch as it constituted the only official codification of law until the 1830s, at which time it was included as the first item in the Complete Collection of Laws of the Russian Empire. The 1845 revision of the criminal code also borrowed from the *Ulozhenie*.

Despite the importance of the subject, and the relative paucity of recent studies of the code, Man'kov's book proves disappointing in several respects. Except for the competent survey of the historiography and the events surrounding the original compila-

tion of the code, the book consists largely of a paraphrase of the *Ulozhenie's* provisions. There is very little in the way of analysis, and virtually no allusions to the real world to which the *Ulozhenie* was directed. Consequently, reading Man'kov's book is often not unlike reading the *Ulozhenie* itself. In summary style Man'kov treats the landholding prerogatives of the service elite, the position of their serfs and slaves, the legal limits of the sovereign's authority and state institutions (among which he counts the Boyar Duma and Assembly of the Land), and finally criminal and civil procedure. What importance any of this had Man'kov fails to say.

Some of the thorniest issues surviving from the nineteenth-century historiography also remain untreated. For example, Man'kov observes that in his view the *Ulozhenie's* compilers borrowed little from foreign (especially Byzantine and Lithuanian) law (pp. 36–38). Nevertheless, the *Ulozhenie* did introduce (via the books of the Robbery Chancellery) talion maimings for thieves. In surveying these penalties (pp. 225–26), however, Man'kov has nothing to say about their origin, despite the fact that one of the books he treats in his historiographical survey (N. I. Tiktin, *Vizantiiskoe pravo kak istochnik Ulozheniia 1648 g.* [1898]) details the Byzantine origin of just these penalties. Indeed, reliance upon Byzantine notions of talion increased with the passage of time, as the 1667 Articles on Criminal Law show. Similar allusions to foreign sources occur throughout the *Ulozhenie*.

Finally, Man'kov's publishers have served him poorly. While the text alleges the importance of the book's subject, the binding and paper cover indicate that the publisher thought otherwise. Several typographical errors also were uncorrected, presumably because of a hasty and superficial proofreading (pp. 72–92 only, to judge from the list of appended corrections). Such low-quality productions are not warranted for major academic books from the main academic press of the Soviet Union.

DANIEL H. KAISER
Grinnell College

GLYNN BARRATT. *Russia in Pacific Waters, 1715–1825: A Survey of the Origins of Russia's Naval Presence in the North and South Pacific*. (Pacific Maritime Studies, number 1.) Vancouver: University of British Columbia Press. 1981. Pp. xviii, 300. \$24.95.

Western historians have generally paid little attention to Russian and Soviet naval history except as it relates to major national and international events. Russian naval activity in the Pacific in the eighteenth and early nineteenth centuries has unfortunately fallen into the neglected category, although it sheds light upon Russian commercial and administrative

problems in eastern Siberia and Russian America and upon the Russian state's perception of the navy's role in international relations. Glynn Barratt has produced a much-needed study of this subject, one that sets new standards for research in Russian naval history.

Barratt has chosen to begin his account with the arrival of the first party of Russian naval personnel at Okhotsk in 1715. As with later naval undertakings, the orders of these men enjoined them to cooperate with and assist local administrative and commercial interests. The Russian navy was not to act as an instrument of imperial might in the Pacific, for the Russian state did not wish to draw the attention of other great powers to the relative weakness of Russia's presence in the area. Naval parties, sometimes disguised as commercial ventures to divert foreign interest, explored the North Pacific littoral, attempting to discover an ice-free passage to the Orient around the top of Asia and charting the coasts of Northeast Asia and Northwest America and the islands in between.

A certain rivalry between naval and commercial groups was in evidence from the first, but the struggle for control began in earnest with the chartering of the Russian-American Company in 1799 and the shift in provisioning Russia's Pacific settlements from overland routes to circumnavigatory voyages. Naval and company officers constantly quarreled over their respective spheres of authority, prompting the navy to launch a successful campaign for control of the company's administration. But the navy's victory was hollow, for the Russian state, realizing that it lacked the power to dominate the North Pacific, concluded agreements with Great Britain and the United States that undermined the company's control of the fur trade in Russian America. Barratt closes his account with these last developments, which effectively eliminated the need for any increased projection of Russian naval power in the Pacific.

Barratt's research is impressive. Besides having made a seemingly exhaustive survey of the printed sources, he has succeeded in gaining access to archival materials from TsGAVMF, the Central State Archive of the Navy; AVPR, the Archive of Russian Foreign Policy; and several other major Soviet archives. His work is obviously the product of considerable informed thought and is very readable. There are one or two minor errors of historical fact, but the book's only real shortcoming is the absence of detailed maps of eastern Siberia, the Aleutians, and Russian America. Barratt's bibliography, which covers the archival sources in detail, is excellent. There are also good separate indexes of people, places, and ships and vessels. All in all, the book is an outstanding contribution to this field of knowledge.

J. DANE HARTGROVE
National Archives

DANIEL T. ORLOVSKY. *The Limits of Reform: The Ministry of Internal Affairs in Imperial Russia, 1802–1881*. (Russian Research Center Studies, number 81.) Cambridge: Harvard University Press. 1981. Pp. viii, 299. \$25.00.

This exceptionally thoughtful and reflective, dense, and careful study of the Russian Ministry of the Interior from its founding to the crisis precipitated by the assassination of the Tsar-Liberator is part of a growing literature in the field that makes original and imaginative use of archival materials.

One of Daniel T. Orlovsky's key concepts is something he calls "the ministerial power ethos" (*passim*). It is here that he locates the unresolved paradox at the heart of nineteenth-century Russian government: the impossibility of reconciling an effectively functioning, securely grounded, stable yet flexible bureaucratic administration with the mystical view of an autocrat of unlimited personal power, an iconic figure presumed to embody in the mystery of his personality the political essence of the empire, to whom the administration was attached and subordinated. Orlovsky describes how under Alexander II the Ministry of the Interior, beginning with its role in the emancipation of the serfs and ending with Loris-Melikov's bold plans for administrative-constitutional reform, became virtually a state within the state. It achieved a preponderance over more technically and professionally oriented ministries like education, justice, and finance largely because of a power ethos shared by the tsar and his ministers that saw the heart of autocratic rule in those tasks of control, regulation, and police in which the Ministry of the Interior specialized.

Orlovsky uses a conventional distinction between "state" and "society"—without rehearsing the historical emergence and development of this distinction—to help to describe the complex maneuverings of the autocracy to tap the energies and enlist the support of new social formations without relinquishing either its claims to absolute power or its presumed capacity to control. This attempt he calls in turn "conservative renovation," and he points out typological similarities as well as differences in detail and degree between government policy in post-emancipation Russia and similar processes going on at about the same time both to the west and to the east. He does not attempt a detailed study in comparative history, but he does suggest the outline for such a study.

He refers also to something he calls "political culture," which enables him to structure the prosopographical studies he also includes into a pattern that relates the talented and the untalented, the rich and the poor, the liberal and the conservative. It is an interesting concept that could bear further elaboration.

By concentrating on the reign of Alexander II,

Orlovsky succeeds in demonstrating that there was *not* in Russia a single "crisis of autocracy," that, in response to the "revolutionary situation" of 1859–61, produced the Great Reforms, which in turn came to a sad end with reaction to the assassination of Alexander II. There was rather an ongoing crisis, visible from the time of the formation of the ministries in 1802, and dramatically manifest throughout the reign of Alexander II, in which ministerial power contested with the rule of law, separation of powers, and local self-government—a contest in which rigid control rather than suppression or elimination was the object of struggle. Loris-Melikov's project was merely a very temporary reversal of the trends of the past two decades.

Although a bit weak in sustaining narrative interest, Orlovsky's book, with its rich use of Ministry of the Interior archives, its clarity and brevity of statement, and its interesting conceptual apparatus, is packed with paths and impulses that should lead productively to future study.

SIDNEY MONAS
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EDWARD C. THADEN *et al.* *Russification in the Baltic Provinces and Finland, 1855–1914*. Princeton: Princeton University Press. 1981. Pp. xiii, 497. Cloth \$40.00, paper \$17.50.

This tome starts out bearing two heavy burdens. The first one is the topic, the second is its organization. Russification is so all-encompassing a phenomenon that to discuss it means to write a general history of the nations subjected to it. But since the focus, for reasons of space, is concentrated, the history remains skeletal. Ideally the authors should have written a history of each nation, Russification included. But that would have led to five separate volumes.

The editor and coauthors, having shared conference panels, decided to combine their efforts within a single volume that posed the problem of repetition. Editor and coauthor Edward C. Thaden, writing on Russian policy, naturally had to discuss its impact upon Baltic Germans and upon Latvians and Estonians in Latvia and Estonia (then Kurland, Livland, and Estland). Michael H. Haltzel, dealing with the Baltic Germans in Latvia and Estonia, could not escape writing about Russian policy as it affected the Latvians and Estonians as well as the Germans and the interrelationships of the former with the latter. Andrejs Plakans and Toivo U. Raun concentrated on Latvia and Estonia but had to rehash matters previously discussed. C. Leonard Lundin was the luckiest, since Finland was less involved than the others in intraregional competition and, except for marginally treated Poland, had the most distinct

sense of national identity. Thaden was aware of the dangers of a side-by-side organization and admits that repetition was unavoidable.

Despite these obvious flaws (which do not dull the readability of Lundin's section on Finland), the book is worthwhile because there is no other comparative study of tsarist Russification by scholars knowledgeable in Latvian, Estonian, and Finnish. Although only the Protestant peoples of the Baltic are viewed in detail, they were the westernmost and most Western elements of a backward empire and hence its least homogeneous portion. Adding interest is the fact that Kurland, Livland, and Estland, dominated by German barons, were but a few days' march from eastward-pressing Germany. Many in Finland, thanks to Swedish influence, were also pro-German, and in any case the Finns stubbornly refused to accept their conquered-nation status. In short, Russification, although a device for imperial unity, was of highly strategic consequence in the Protestant regions of the Romanov empire. Russification also contributed greatly to the intensity of the 1905 revolution, especially in that area, a subject which deserved more attention than it received. The book is of further value in demonstrating how relatively humane was the much-demeaned tsarist Russification, as compared to that of the Soviet Union, where, in practice, it has often amounted to genocide.

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V. P. DANILOV. *Sovetskaia dokolkhoznaia derevnia: Sotsialnaia struktura, sotsialnye otnosheniia* [The Soviet Village before Collectivization: Social Structure, Social Relations]. Moscow: Nauka. 1979. Pp. 357. 4 r.

V. P. Danilov, a leading Soviet specialist in the history of twentieth-century peasantry in the USSR and author of numerous works in this field, offers here his second remarkable volume, with one yet to come in his trilogy on the Soviet pre-kolkhoz peasantry. The earlier volume published in 1977 dealt with population, land use, and farming. The one now under review deals, as the title suggests, with the social structure and social relations in the countryside, mainly during the 1920s, and, not unexpectedly, it focuses on the problems of economic differentiation and social stratification of the peasantry. The economic factors that contributed to inequality in economic strength and well-being of peasants, such as distribution of means of production, lease of machines and of draught animals, renting of land, employment of wage labor, and rural market activities, are studied in such unusual detail and wealth of information that one is tempted to say that there is nothing more that can be added to Danilov's presen-

tation except, of course, a different interpretation. The same applies to the study of the different forms of cooperation, including the collective farms, as they existed before the big "collectivization drive."

As just hinted, the analysis and interpretation of the whole material, especially as it emerges in Danilov's last chapter on "class structure of the soviet *pre-kolkhoz* village," is the most problematic in this reviewer's eyes. The author seems to be torn between two contradictory tendencies to which the material and the political realities, of the countryside and of the regime, contribute their respective parts. His interpretation seems to waver between emphasis on the benefits of a revolution that substantially curtailed the richer strata and created a more homogeneous, less differentiated social structure in the village, and the emphasis on "classes" inside this peasantry, especially on the kulaks, who managed, supposedly, to have become such a menace that their destruction was warranted despite the fact that they were three to four times weaker than before the revolution.

It is impossible to enter into a substantive discussion in a short review, but it has to be underlined that Danilov—a man of enormous learning and impressive command of the material—gives us in his work all we need to make up our mind about the problems at hand. This, no doubt, is a sign of serious scholarship. He is quite cautious in his formulations, and he also helps us very much by assessing the quality of the sources used in his volume, especially of the statistical data that are available. It is the examination of those sources and the methods their authors used, especially for discovering and sizing up classes in the countryside, that show us how well Danilov understands rural realities and the complexities involved in penetrating their apparent simplicity. A healthy skepticism is a must in this domain—and Danilov's book conveys it to us in many ways.

All in all, this is an important contribution by an outstanding scholar.

MOSHE LEWIN
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ALEXANDRE A. BENNIGSEN and S. ENDERS WIMBUSH. *Muslim National Communism in the Soviet Union: A Revolutionary Strategy for the Colonial World*. (Publications of the Center for Middle Eastern Studies, number 11.) Chicago: University of Chicago Press. 1979. Pp. xxii, 267. \$7.95.

To an orthodox Marxist, national communism is a contradiction in terms. Yet national communism has been a part of the communist movement at least since the opening of the twentieth century. In Russia the national problem reared its head within the socialist movement long before the October

Revolution and has continued to be a live issue to this day. Alexandre A. Bennigsen and S. Enders Wimbush in their *Muslim National Communism in the Soviet Union* offer a brief history of the "nationalist deviation" among Russia's Muslims who joined the socialist-communist cause.

Muslim peoples constituted some 15 percent of the population of the tsarist empire. Those members of their elites who chose not to remain either passive or subservient to their Russian masters tended to become socialists. Bennigsen and Wimbush convincingly argue that it was not doctrine that attracted the Muslims. Rather they found socialism appealing as an instrument of conspiracy and mass mobilization. Socialism was also attractive because it promised outside support in what was essentially an anticolonialist struggle and because it gave hope of "equality, if not outright independence" (p. 14).

The relatively small body of Muslim communists that emerged from the ordeal of the revolution and civil war rapidly lost its illusions about communist internationalism. Muslims, whether Tatar, Uzbek, or Azerbaijani, found the party and the Soviet state run by and for the Russians and concluded that socialism did not necessarily bring the end of imperialism. Mir Said Sultan Galiev, a Tatar colleague of Stalin's at the People's Commissariat of Nationalities, declared as early as 1919 that the Bolshevik revolution had not eliminated Russian imperialism. "The hegemony of the Russian people over other nations," he proclaimed, "must be replaced by the dictatorship of these same nations over the Russians" (p. 47).

Class differences, Sultan Galiev taught, applied not only within but also among nations. Imperialist nations were imperialist through and through. In relation to India even the British workers were imperialists. Moreover, there would be no revolution in the West if it were left to itself. Lenin's notion of the "weakest link," which he used to legitimize the supposedly proletarian revolution in a heavily agrarian society, meant that those who were the most oppressed were the most revolutionary, and who was more oppressed than the colonial peoples of the world? It is they who would ignite the fire of world revolution. Without them, Sultan Galiev declared, "it is impossible . . . to achieve the international socialist revolution" (p. 53). Therefore Muslim communists advocated maximum involvement in anti-Western revolutionary movements in China, Turkey, and Iran. The Russian leadership could not allow such ideas to spread and become dominant in the Communist party. Sultan Galiev was jailed in 1923. In the 1930s virtually all prominent national communists—Tatar, Uzbek, Kirghiz, Azerbaijani—perished in the Great Terror.

Yet their ideas survived not only in the Soviet Union but also abroad. One finds echoes of national

communist opinions in the writings of Chinese communists and among communist and socialist leaders of many nations of Africa and Asia. It is noteworthy that Algeria's Ben Bella was familiar with Sultan Galiev's thought and held it in high esteem. In many pre-industrial societies there existed conditions similar to those that had earlier set the Muslims of the Russian empire on their peculiarly unorthodox Marxist course.

Bennigsen, who is well known for his earlier work on Islam in the Soviet Union, and Wimbush have produced a brief study of an important and relatively unexplored subject. They have placed in a long appendix translations of some of Sultan Galiev's speeches and articles as well as a number of other documents that are otherwise unobtainable in English.

It is a pity that such a valuable book is so disfigured by careless writing and poor editing (including bad grammar, sloppy spelling, and inconsistent transliterations) and an array of minor errors so numerous that to list them would require more space than has been allotted to this review. The Chicago University Press ought not to escape its share of the blame for abandoning the standards of performance one has the right to expect of an academic publisher.

F. KAZEMZADEH
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MARSHALL S. SHATZ. *Soviet Dissent in Historical Perspective*. New York: Cambridge University Press. 1980. Pp. x, 214. \$19.95.

Marshall S. Shatz's is a popular book in a rather unpopular genre in modern American intellectual discourse. It does not pretend to be based on fresh archival discoveries (which would make it sound like a historical study) or on new data and new techniques of analysis (which would make it relevant for a political scientist). Instead it attempts to "pick out long-term configurations of human behavior in a changing society" in order to "bring the findings of the past to bear on the present." Such an attempt will probably make both the historian and the political scientist extremely unhappy. Nevertheless, it seems to bring results. One of these is that "for all the vast social, economic and political changes that have swept over Russia since 1917, the social sources of Soviet dissent are strikingly similar to those that had begun to generate the Russian intelligentsia at the end of the eighteenth century."

To those who would object that there is actually little new in such a conclusion, I will reply (I hope along with the author) that if we had paid more attention to these long-term patterns of political behavior in autocratic society, we obviously would have been able not only to envisage the emergence

of Soviet dissent long before it actually emerged but also to predict exactly which social groups were destined to be involved in it. For this we simply had to look (which we did not) for a Soviet equivalent of the educated elite in imperial Russia. By such an equivalent Shatz apparently means groups placed in "a relatively privileged position" that "permitted . . . the rise of a new consciousness of individual worth, generating a demand for greater autonomy from the state controls," whom "education enabled . . . to articulate their grievances" and whom a "strong sense of service to their society impelled . . . to speak out despite the obvious risks of such behavior." It goes without saying that Shatz is speaking of the Soviet writers and scientists who indeed occupy the predominant position in the Soviet dissident movement.

This is only one example of Shatz's unconventional but rather fruitful attempt. There are many others, the most significant of them being his discussion of "the curious pattern of reform and reaction," the cycles of dynamism and retrenchment that, to our surprise, have marked both the behavior of the imperial and Soviet governments for centuries.

Of course, this new (or long-forgotten) genre does contain some intrinsic, built-in traps for any author choosing such a strange and fascinating path. Unfortunately Shatz did not escape them. Relying too heavily on the available historical sources, he sometimes repeats uncritically their many clichés, some of which seem more than dubious. For example, he connects the emergence of Russian autocracy and serfdom with "the constant necessity of warding off all [foreign] threats." Or he praises this autocracy as "a dynamic and progressive force." Or he begins the history of Russian dissent from the time of "the Westernizing reforms of Peter the Great."

In my book *The Origins of Autocracy: Ivan the Terrible in Russian History*, which is about to be published by the University of California Press and which is written, I must admit, in the same unconventional genre as Shatz's work, I have tried to show the first fierce eruptions of Russian dissent in the 1480--90s. It would be hard by any stretch of imagination to connect it either with Peter's reforms or with the "Western-educated nobility" that, according to Shatz and the historical clichés he so innocently trusts, is supposed to be the sole source of dissent in Russia.

Nonetheless, Shatz's book seems to me an important beginning that could mark a significant breakthrough in Western studies of Russia and particularly in ending the unbearably boring "civil war" between historians and political scientists.

ALEXANDER YANOV
University of California,
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ADAM B. ULAM. *Russia's Failed Revolutions: From the Decembrists to the Dissidents*. New York: Basic Books. 1981. Pp. vii, 453. \$18.95.

Adam B. Ulam's latest book on the development, realization, and transformation of revolutionary ideas and groups in Russia and the Soviet Union focuses on the issues of freedom, nationalism, power, and authoritarianism. He devotes the bulk of his study to moments and periods in Russian revolutionary history when there were both illusions about and real opportunities for seizure of power by revolutionary organizations. Thus there is a long chapter on the Decembrists, a chapter summarizing the nihilist and *narodnik* movements and the assassination of Alexander II, three chapters on the period 1905–17, and one on the period 1917 to the present. Ulam stresses the inability of revolutionary groups from the Decembrists to the Kadets, Mensheviks, and SRs to seek power vigorously, without crippling fears and ideological constraints. He provides for each era the larger context in which the intelligentsia failed to deal decisively and pragmatically with the concrete problems that it faced and with the autocratic power. The theme of Russian nationalism and the hesitancy of the intelligentsia to destroy the autocratic power that sustained the Russian empire recurs in conjunction with the problems of oppressed minorities and nationalities, especially those of Jews and Poles. When a scholar who has already written several books covering most of the areas described above produces a sizable new study, it usually means that new concerns and new purposes inform his treatment of the subject. Although Ulam's previous studies have always been well informed, they have never been monographic. The same is true of his latest work. Thus it is more profitable to deal with his new concerns and purposes than with his scholarship.

Ulam argues that at crucial moments fear of power and anxiety about Russian national greatness stayed the hand of revolutionaries confronting the old autocratic power. Furthermore, once a revolutionary government appeared, it failed to protect itself against Lenin's counterrevolution, which recreated autocracy and (eventually) Russian national greatness and power. Stalin's power hunger and the Communist party's quest for power cloaked themselves in patriotism. Thus Ulam tells a story of autocratic power, national greatness, and lack of freedom in a continuous process from the Muscovite tsars to the present, with periods of relatively freer expression and participation of "society" in affairs of state. But the new autocracy has studied the revolutionary history leading to its own victory and is far better at suppressing dissent than the last tsars were. The brakes placed on liberalization dur-

ing the Khrushchev and Brezhnev regimes reveal the deeply autocratic character of the communist state, despite their repudiation of Stalinist mass terror.

Ulam believes that so long as Russia's new autocrats continue to succeed against less resolutely competitive democratic states, Soviet citizens will continue to support their leaders against dissenters and bear up under sacrifices made for national greatness. Should Western democracies stand firm, following Solzhenitsyn's prescription, then Soviet dissidents, like their antecedents in the revolutionary intelligentsia, might live to see a moment when a Soviet state in retreat internationally and in difficulty domestically might falter in the eyes of its citizens and provide another occasion for freedom to emerge. Ulam rejects the idea of the convergence of the Soviet system with those of modern democratic states and suggests that Russian Soviet autocracy can only be brought down by a combination of its own failures and the competitive successes of Western-style democracies. The question remains: Who will survive the competition?

PHILIP POMPER
Wesleyan University

KENNETH C. FARMER. *Ukrainian Nationalism in the Post-Stalin Era: Myth, Symbols, and Ideology in Soviet Nationalities Policy*. (Studies in Contemporary History, number 4.) Boston: Martinus Nijhoff. 1980. Pp. x, 241. \$36.50.

Kenneth C. Farmer's provocative study of the Ukrainian dissident movement is a prime example of the analytical difficulties that unavoidably arise when a procrustean conceptual scheme is imposed on a well-constructed data base. Central to Farmer's conceptualization is that the Soviet nationalities problem is "at root" a question of the irreconcilability of two "myths"—"proletarian internationalism" and "national self-determination." Thus, "under conditions of severely restricted communications," such as those prevailing in the USSR, the "most important" form of conflict between the regime and its dissident critics is "symbol manipulation."

These sweeping theoretical generalizations are followed by a thoughtful analysis of the various symbols and myths being manipulated by both sides. Farmer perceptively discusses Soviet nationalities policy, the Ukrainian national-cultural revival, the status and symbolic importance of the Ukrainian language, and the size, scope, and ideology of the dissident movement. Especially welcome are his analyses of the intricacies of the "Soviet *narod*" debate and of the regime's antidissent measures.

Yet despite these merits, Farmer fails to prove his main point: that symbolic analysis is the key to

understanding Ukrainian nationalism. By reducing "nationalist opposition and regime response" to a question of symbol manipulation, Farmer underestimates the no less important historical, political, and economic dimensions of this confrontation. In the process, contemporary Ukrainian nationalism is severed from its roots in the recent Ukrainian past, and the centuries-old problem of Ukrainian-Russian relations is treated largely in terms of the "myth of Russian primacy." Moreover, the dissidents are made to appear conscious of the symbolic value of their actions. While this may be true of such activists as Valentyn Moroz, it is clearly an exaggeration to ascribe such motives to the authors of most of the petitions and complaints that circulate in *samizdat*. Farmer recognizes that "Ukrainian nationalist dissenters have attempted to exhaust all legal means of redress before resorting to symbolic action" but fails to draw the conclusion that this is as much as to say that symbolic action is but one aspect of dissent and, analogously, that symbolic analysis is useful only as a conceptual tool of limited value. Likewise, Farmer's awareness of the fact that the Ukrainian dissident movement was crushed "through coercion and symbolic action" justifies questioning his own claim that symbol manipulation is the most important form of regime response to dissidence. After all, is the sentencing of a dissident to a ten-year term in a strict-regime labor camp really primarily an example of symbolic action?

In essence, Farmer has written two books: the first, a theoretical work, suggests an interesting but limited approach to studying modern Ukrainian nationalism; the second, an insightful empirical analysis, would have greatly benefited had its author not tailored it to the limitations of the first.

ALEXANDER J. MOTYL
Columbia University

NEAR EAST

MARK R. COHEN. *Jewish Self-Government in Medieval Egypt: The Origins of the Office of Head of the Jews, ca. 1065–1126*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1980. Pp. xxi, 385. \$27.50.

The documents of the Cairo Geniza, uncovered during an accidental cave-in, continue to provide revolutionary insights into medieval Jewish history. Mark R. Cohen, using Geniza materials as a critical standard, succeeds in demonstrating that the traditional theories accounting for the origins of the nagidate in medieval Egypt, theories based primarily on literary sources, are inadequate. He begins

with a historical review of these theories, earlier criticisms, and a preliminary showing of the flaws that indicate the necessity for revision.

Cohen then presents the Fatimid context within which the nagidate developed and relates that political reality to another model of minority communal structure, the Coptic Patriarchate. He then delves at some length into the sociological factors relevant to the development of local self-rule. Among these he emphasizes the maturation of a strong local leadership and the decline of the Palestinian Yeshiva, which had previously been the accepted and sanctioned head of Fatimid Jewry.

The next four chapters trace the beginnings of the "Headship of the Jews" centering on the rule, deposition, and restoration of Mevorakh b. Saadya. The survey begins in the 1060s (p. 28, l. 27 read 1060s) and ends in 1126 by which time the classical powers of the headship had been established and the office was permanently removed from the house of Mevorakh.

The final chapter articulates Cohen's primary thesis. This asserts that the headship had its origins during the last third of the eleventh century within a "complex web of internal and external circumstances." The office was not created and imposed by the Fatimids although their sanction of it was probably motivated by their own political priorities. The headship, moreover, was not a sudden innovation. It evolved within the context of the tension created by the deterioration of the Palestinian Yeshiva and the maturation of Egyptian-Jewish leadership.

The book ends with two useful appendixes. The first is a listing of the Geniza corpus (with citations given for those documents previously published). The second contains six translated and annotated Geniza documents relating to the headship. There is a bibliography, general index, and index of Geniza texts.

There can be no question that Cohen's primary thesis is well taken and amply shown. In particulars, however, the book is not without weaknesses. Many assertions are made, for example, that are not adequately justified (pp. 94, 101, 219, 229). There is some inconsistency in criticizing the speculative nature of earlier scholarship while replacing it, in part, with further speculation, however reasonable, presented in the language of certainty. Perhaps the most obvious instance is the speculation offered by comparison with the experience of the Coptic Patriarchate (pp. 77, 84, 259, 290, 291). While such a comparative approach is potentially fruitful, it is also dangerous. Cohen himself notes the difficulty in comparing Christian and Jewish minorities in Muslim Egypt (pp. 53, 68).

In all, however, Cohen has made a very important contribution. He has brought together an impressive amount of primary material and put it into a

generally reasonable and cohesive structure. If this book raises many questions and calls for further thought, that, in itself, is an accomplishment worthy of a scholar.

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POLYVIOS G. POLYVIU. *Cyprus: Conflict and Negotiation, 1960–1980*. New York: Holmes and Meier. 1980. Pp. ix, 246. \$44.50.

Polyvios G. Polyviou, who evidently had access to many primary sources, offers a detailed background on the Cyprus talks of 1967–71 and 1972–74, as well as on the Geneva Conference of 1974. The 1967–71 round of intercommunal talks led nowhere because “even when agreement seemed near . . . a great gulf of irreconcilable objectives and opposing philosophies divided the parties” (p. 87). The Greeks wanted to keep the island as a single political entity, the Turks wanted a piece of the island to themselves. The resumption of talks in 1972 with the participation of UN representative Ozorio Taftali proved more promising. The talks resolved many thorny constitutional problems. Polyviou convincingly argues that settlement was within sight when the Greek junta engineered a coup against President Makarios in July 1974, thus unwittingly opening the gates to the Turkish invasion forces. One is tempted to speculate whether the pro-Enosis diehards, alarmed at the potential resolution of the conflict, decided to intervene.

This confusion of objectives has been the hallmark of the Greek policies on Cyprus from the beginning. Some wanted Enosis, others wanted an independent republic dominated by the Greek Cypriots, a few were willing, in the face of reality, to accept some form of bicomunal structure. “What all these divisions meant was that there could be no political consensus among Greek Cypriots as to how best to approach the problem and therefore no agreed policy” (p. 146). As a result, the Greek side relied through the years on the “justice” of its cause, the various factions making one miscalculation after another, never soberly taking into account the realities of power. After each setback, the Greek side was willing to accept what they had previously opposed, only to find that time had already left behind the possibility of an agreement on the basis of what had been acceptable before to the Turkish side. Polyviou’s account shows that this pattern was repeated at the Geneva Conference while the Turkish invasion force still occupied only a very small area on the island. The Turks pressed for a federal structure with “two federated states.” The Greek side tried to hold on to a “unitary” state (p. 172) even though the Soviet Union declined any help (p. 177), the British

would only put their troops stationed on Cyprus at the disposal of the UN, and the Americans were obviously leaning toward the Turkish side. The conference collapsed, giving the Turks the excuse they wanted. Their tanks were already waiting to fan out and occupy 40 percent of the island, drive 200,000 Greek Cypriots from their homes, and achieve the long-standing objective of dividing up the island. Polyviou, who was present at the conference, provides valuable details and a well-reasoned analysis.

At present, the Greek side appears willing to accept some form of a federal state, and Polyviou includes in the closing pages of his book a discussion of arrangements and alternatives. With almost half of the island under Turkish occupation, however, Ankara has no compelling reason to seek a settlement—except maybe as a means of transforming its *de facto* occupation into a *de jure* arrangement. Whatever the future course of events, Polyviou’s book should be read carefully by those who wish to see an equitable and peaceful resolution of the Cyprus problem.

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FARHAD KAZEMI. *Poverty and Revolution in Iran: The Migrant Poor, Urban Marginality, and Politics*. New York: New York University Press; distributed by Columbia University Press, New York. 1980. Pp. 180. \$17.50.

Farhad Kazemi has made an important contribution to the appreciation of the social and economic circumstances of an important segment of Iranian society—the migrant poor who moved into the large urban centers of Iran in the post–World War II era.

This volume discusses the general background of urbanization in Iran, including the origin of the migrants and the nature of Islamic cities. Kazemi demonstrates the importance of rural-urban migration by citing useful figures on the size of such movements, and he gives a concise account of the reasons for the rapid rural-urban migration that occurred in Iran. But the important contribution and result of the study is the analysis of extensive interviews conducted by the author in Iran.

Kazemi has used his data to give the first systematic analysis of the conditions of poor migrants who had become an increasingly large component of the urban population, especially during the 1970s. His vivid depiction of the migrants’ social and economic conditions, coupled with his survey results, clearly demonstrate one of the major failures of the government. The preoccupation of the government with large and visible projects had its counterpart in the neglect of human conditions and the fabric of

society. There is an important lesson in all of this for other Middle Eastern countries that have embarked on the process of rapid economic development at an unprecedented pace. Kazemi further increases the scope of his manuscript by citing pertinent comparisons to the experience of other developing countries.

Kazemi discusses the role of the migrants in the revolution but does not use his results to assess the detailed impact of migration on the Iranian revolution. Nonetheless, the results of his surveys and further research would be very helpful in assessing the migrants' role. All in all this book should be read by all those interested in the process of rural and urban migration, in the pitfalls of rapid economic developments, and in the recent history of Iran.

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AFRICA

ROLAND OLIVER and ANTHONY ATMORE. *The African Middle Ages, 1400–1800*. New York: Cambridge University Press. 1981. Pp. viii, 216. Cloth \$29.50, paper \$9.95.

This is the second part of a textbook trilogy for beginners. The dominant hues are states, trade, and Islam; the canvas takes in everything on the whole continent, barring the sizable Kuba state. The muted greys are the Atlantic slave trade and European activities along Africa's shores. In a nutshell, Roland Oliver and Anthony Atmore hold that between 1400 and 1800 the population increased as a result of better "later iron age" technology and that an enlargement of scale in political groupings ensued. Needed labor for this growth was slave labor acquired in war, in raids, and in the payment of debts. Even at the height of the Atlantic slave trade there were many more African slaves on the continent than outside of it. The slave trade was but a by-product of the main forces of change (pp. 195–97).

The title is defended by the claim that the sources of our knowledge were better and more varied after 1400, whereas the states and cities that then grew up were quite comparable with those of medieval Europe (pp. 1–4). The structure is simple: the authors take us, after a sketch of Africa ca. 1400, on a tour around the parts of the continent in thirteen chapters.

This book is very well written indeed. The parts dealing with North Africa are the best; other parts vary in quality roughly according to the research carried out in a particular region—the segment on northern equatorial Africa comes out the weakest. The nadir of the book certainly occurs on pages

114–19, where almost nothing can withstand critical analysis. A few brush strokes cover a great historiographical blank: the savanna and the rain forests of equatorial Africa from about 12°N to 5°S. This is a huge part of Africa. To fill in the gap, the authors use language groups as a sort of surrogate state, one that expanded and retreated, as if common origins of speech implied common goals, common action, and common interests. The gap, in fact, displays how the authors interpret the past better than in other sections, where they chose the data with consummate skill so that their choices were never obvious.

The maps are exceptional, and no other text is so well served. But they are undated, a fact that leads to anachronisms. They also turn into fact whiffs of hypotheses. Thus on page 21 we see only Mongo and pygmies in the forest, and even for 1400 that is indefensible. Where the text is weak, the maps are worse (see, for example, pp. 115, 137, 151).

The stress on states is far too pronounced for me, as if there were not so many kaleidoscopic transitions between one structure and another and so few "states" that conformed to even approximate European notions, the notions the youthful readers will bring to the text. Where are the deep trends, the slow-moving forces? Braudel is invoked in this text but not followed. Too often generalizations are a mere condensation of data while flames of imagination leap across the pyres of available fuel. And neither the "Middle Ages" nor the beginning date of ca. 1400 are really acceptable. Nevertheless, the elegance of this view challenges, and, along with other texts of recent vintage, this too has a place on the shelf for beginners.

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ALBERTO SBACCHI. *Il colonialismo italiano in Etiopia, 1936–1940*. Foreword by ROMAIN RAINERO. (Biblioteca di Storia Contemporanea, number 20.) Milan: Mursia. 1980. Pp. xii, 357. L. 20,000.

Utilizing unpublished documents from Italy's African Ministry and Central State Archives, Alberto Sbacchi has written a comprehensive and valuable account of the five-year Italian colonial venture in Ethiopia. It includes a great deal of information for anyone intrigued by the larger problems of Fascist Italy, Haile Selassie's Ethiopia, or European colonialism in Africa. The book was originally written in English and was translated by Romain Rainero, who also contributed a historiographical introduction.

Il colonialismo italiano in Etiopia is divided into three equal parts. "The Italian Administration in Ethiopia" characterizes the colonial system as inade-

quately planned, poorly organized, and crudely implemented by persons with little knowledge, training, or concern for local conditions or colonial complexities. Personal antagonisms and rivalries among the leading figures, racial arrogance and abuses of power by lesser officials, and everywhere the heavy hand of Rome in political and financial decisions complete the picture of a demoralized Italian colonial administration and a resentful Ethiopian population.

Originally, Mussolini had expected to achieve via a show of force and diplomacy a protectorate-type arrangement, which would work through Haile Selassie and the traditional *rases*. "Conquerors and Conquered" shows how the Italians, once in control in Addis Ababa, reversed this policy and opted for direct rule. Stripping the Amhara *rases* of their power and status and favoring Muslims and Gallas over the Christian Amharas galvanized and unified the patriotic resistance forces, which the Italians could never overcome despite claims of Fascist triumph and invincibility. Haile Selassie had sought to reduce gradually the power of the *rases*, but the abrupt Italian policy outraged the nobility and put them on the patriotic side. Sbacchi argues that if d'Aosta's more sensitive and accommodating policies had been implemented, Italy might have overcome Ethiopian hostility, but his short rule, precarious health, heavy duties, and constant conflict with the Ministry for Africa in Rome prevented this.

"Italian Colonization in Ethiopia" catalogues the failure to settle any significant number of Italian farmers in Ethiopia, ostensibly one of the reasons for the colonial undertaking in the first place. The immense cost of colonial resettlement programs, military insecurity due to the inability to put down resistance groups or to complete the conquest, and the lack of interest among Italians combined to destroy Italian hopes.

Sbacchi is objective in analyzing the myriad problems and failures of Fascist Italy in conceiving, organizing, and carrying out the Ethiopian venture. He suggests that Ethiopians made a distinction between Fascists and Italians in general. Taking into account "the advantages of western life" (p. 346), such as roads, technology, and crops, that Italy had brought to Ethiopia, and despite the memory of Graziani's reign of terror, Ethiopians carried out no policy of revenge against Italy after the Anglo-Ethiopian victory in 1941. Using Italian efforts to centralize authority, Sbacchi concludes, Haile Selassie more easily re-established his own power. "His own subjects in the end found him more oppressive than the Italian occupation" (p. 347).

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WILLIAM SPENCER. *Algiers in the Age of the Corsairs*. (Centers of Civilization Series, number 34.) Reprint.

Norman: University of Oklahoma Press. 1976. Pp. xv, 184. Cloth \$5.95, paper \$3.95.

On July 5, 1830, the tricolor was proudly hoisted over the Kasbah of Algiers following its surrender to the French army. Thus came to an abrupt end Turkish rule in the regency of Algiers, the "Scourge of Christendom" whose formidable corsairs had plagued European shipping for over three centuries. William Spencer's admirably concise and highly readable book, which covers the political, social, and economic aspects of the regency, provides a long overdue corrective to the prevailing view of Turkish Algiers as a barbaric predatory state with few, if any, redeeming features. To general reader and scholar alike (although the latter may regret the lack of footnotes), Spencer shows that the remarkably accommodating Turkish regime provided the regency with a well-developed and highly defensible capital city, a smoothly run administration, a harmonious social system, a prosperous economy, and a masterful foreign policy. The Turks thus bestowed on the Algerine population a viable and stable state that would have doubtless continued to flourish if the French had not supplanted it in 1830 with their exploitative colonial regime.

The Turks had managed to base the regency's economy on sound foundations. On the one hand, the corsairs brought in huge revenues through "the cargoes of prizes taken at sea, the ransom of captives, and tribute paid by various European nations under formalized treaty arrangements to protect their ships from corsair seizure" (p. 112), while concurrently carrying on lucrative legitimate trade with Europe. On the other hand, Algerine agricultural production provided not only for domestic needs but also for substantial and highly profitable exports. These two pillars of Algerine economic prosperity—the sea and the fields—made possible the other achievements of the regency enumerated above. Until 1830, the Turks thwarted all European attempts to crush the regency through a shrewd policy of *divida et impera*.

In his commendable endeavor to rectify the regency's slanted record, the author may have moved a bit too far in the opposite direction. The Algerine Turks actually ruled as much by coercion as by accommodation. They secured indigenous submission and respect by administering merciless collective punishment for the slightest infraction. While occasionally promoting indigenous agricultural production, they ruthlessly exploited it most of the time. Massive marabout-led tribal revolts, which followed steep increases in the internal tax burden intended to supplement the dwindling privateering revenues, shattered the regency's tranquillity during the first two decades of the nineteenth century.

These brief comments are in no way intended to detract from the high quality of this book. It is

certain that if, as has commonly been assumed, the vastly outnumbered Turks had indeed relied on force alone, they would have been unable to maintain their rule over the indigenous population for so long. On the basis of original documentation, Spencer has convincingly demonstrated that the Turks established in Algiers an eminently successful state, far superior in almost every respect to its French successor; he has thus rendered a valuable service to all serious students of the Maghrib.

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ARNOLD H. GREEN. *The Tunisian Ulama, 1873–1915: Social Structure and Response to Ideological Currents*. (Social, Economic, and Political Studies of the Middle East, number 22.) Leiden: E. J. Brill. 1978. Pp. viii, 324. f 96.

In a timely historical and analytical study, Arnold H. Green discusses the status and traditional role of Tunisian *ulema* during a period portending radical sociopolitical changes. He shows how this role has been their prerogative by virtue of their standing in Muslim society. From this we learn that their views toward radical change are preconditioned by ideological perspectives molded in the caldron of inherited Islamic values. His introduction is particularly useful in providing a perspective that helps the reader understand the attitude of the *ulema* in Tunis toward events in the critical decades embracing attempted reform and culminating with the French occupation.

It is evident that in Tunisia, and the Ottoman empire at large, the *ulema* tended to uphold views mirroring Islamic society's attitude. They challenged would-be reform measures if public opinion decreed it. While the upper *ulema* tended to support measures introduced by prevailing authority, the lower *ulema* more often than not resisted and held tenaciously to traditional values. The disparity in attitudes mirrored disparity in social backgrounds, which the author amply illustrates with statistics and charts. Yet neither group was prepared to compromise basic provisions of the *Shari'a* (Islam's fundamental law) in the face of proposed change deemed contradictory to the time-honored norms upholding Islamic society. By and large, both elements opposed reform generated either by Western-influenced modernists or by Tunisian rulers, especially during the French occupation of the country. They endorsed change only if it was conceived in the framework of honored Islamic traditions, not at the expense of those traditions. They wielded the moral authority to enforce their views by virtue of their role as custodians of Islam's most sacred law, the *Shari'a*.

Green focuses on the *ulema's* considerable role in shaping educational and legal policy because this was their traditional prerogative. He supports their extensive commitments in this regard with numerous charts, which shed light on their social backgrounds, training, and, above all, determinants of their attitudes. He renders a difficult subject more understandable with detailed lists and figures providing supportive information with regard to specific roles occupied by the *ulema* as instructors in the prestigious Zaytuna mosque and other institutions of learning, and as judges in the provincial courts. He also points up the divergent attitudes of the *ulema* due to economic and social stratification ensuing from different backgrounds. This is clearly evident in the very extensive appendix detailing the biographies of the *ulema* derived from known sources and the author's own personal interviews.

This book is a distinct contribution to the study of Muslim institutions likely to influence and shape future trends in Islamic lands.

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JANET L. ABU-LUGHOD. *Rabat: Urban Apartheid in Morocco*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1980. Pp. xxii, 374. Cloth \$30.00, paper \$12.50.

During the period of French colonial rule in Morocco, the administrators in charge of urban planning consciously established and pursued policies similar to the apartheid policies of the Republic of South Africa. This is Janet L. Abu-Lughod's central thesis, one that she argues effectively and pursues throughout the colonial and into the independent period of Morocco's modern history. The author demonstrates that one of the legacies of the half-century of French rule in Morocco is a caste system that divides Moroccans along social and economic lines that vividly remind observers of the racial (as well as social and economic) segregation of the colonial period.

In developing her thesis, the author has also presented a clear and readable account of how Rabat and, to a lesser extent, Rabat's twin city, Salé, have come to be what they are. Abu-Lughod also asks a question about the future: Will Rabat maintain the social gaps inherited from the colonial period, or will the moral unity that characterized the city before the French conquest be re-established?

Although only the future will see an answer to this question, the basic thesis advanced in this excellent volume will convince most readers. If some object to specific terms such as *apartheid*, which presumably describes a special situation in South Africa, or *caste*, which many would apply only to a peculiar social organization in the Indian subcontinent, no one will

fail to appreciate the history of urban planning and the careful look at the development of class divisions in a Moroccan city. The author clearly demonstrates that this class division arose because of French colonial administrative policies. For historians of the Maghrib, this volume will force a careful re-evaluation of Resident General Lyautey—a colonial hero of long standing because he supposedly protected the natives. After reading this book, most will see that his paternalism was not even well-disguised racism.

An earlier book, Kenneth Brown's *People of Salé* (1976), introduced specialists interested in North Africa to urban studies. Lawless and Blake's *Tlemcen* (1976) followed. Abu-Lughod's *Rabat* is an excellent addition that should be welcomed by historians and social scientists and that, one hopes, will encourage further studies—on Marrakesh, Fez, and Meknes in Morocco; on Algiers, Oran, and Constantine in Algeria; and on Tunis and a number of other cities in Tunisia. *Rabat* is certainly one of those books that approaches the history of North Africa on its own merits instead of in terms of predetermined interpretive prejudices. Many new studies and an equal number of re-interpretations are still needed.

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VIRGINIA THOMPSON and RICHARD ADLOFF. *The Western Saharans: Background to Conflict*. Totowa, N.J.: Barnes and Noble or Croom Helm, London, 1980. Pp. 348. \$27.50.

When Spain abruptly pulled its armed forces from the Spanish Sahara in 1975, all the states neighboring on the area tried to fill the vacuum: Morocco historically considered the territory to be part of a Greater Morocco, Mauritania called attention to the close ethnic ties that the Saharan tribes had to the Mauritanian people, and Algeria gave a sanctuary in Tindouf to the nationalist Polisario Front that claimed the right to set up an independent state in the western Sahara. Libya, under Moamar Khadafi, supported the Polisario in hopes that the territory could become part of a great Arab Maghrib (North-west Africa).

Virginia Thompson and Richard Adloff's book has a chapter on Mauritania's development and interest in the former Spanish land and also a chapter dealing with Morocco's intense interest in getting the Spanish Sahara. There is a separate chapter on the historic and economic evolution of the Spanish Sahara itself. After the authors treat these topics at length, they describe the Saharan war between the Polisario Front and Morocco currently being fought, as well as Mauritania's withdrawal

from the war and the coincident coup d'état that toppled President Mokhtar Daddah.

There are sections on contemporary events in the western Sahara and world reaction to them, especially the attitudes of the United States and Russia. Neither of the superpowers wants to be drawn into a major confrontation in the area although, in general, the United States is supporting Morocco and Russia is in sympathy with the Polisario Front and their ally Algeria.

Spanish and French relationships in Morocco figure importantly in the story. Because of this, it is surprising that the book does very little with the Rif War and the struggle of both Spain and France to subdue the native forces in Morocco led by Abd El Krim.

The book includes useful material in the form of maps, a list of acronyms, and a glossary. Although the work includes an extensive bibliography, there is no division between books, periodicals, and government documents. In general the bibliography is very weak in primary material, especially government documents.

The book ends with an appendix that describes the main tribes and tribal confederations in the western Sahara. This seems anticlimactic. It would be better to include this material in its proper place in the main text. Nevertheless, this book furnishes valuable information on the western Saharans. It is one of the very few recent works in English on this important area.

The western Sahara contains some very interesting tribes, oases, and caravan centers. The authors describe the area but fail to get the reader caught up in the sights, colors, smells, and overall excitement of a fascinating society. There is no evidence that they visited the area, and that makes a big difference—perhaps the crucial difference between a good book on the subject and an excellent book.

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T. G. O. GBADAMOSI. *The Growth of Islam among the Yoruba, 1841–1908*. (Ibadan History Series.) Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. xv, 265. \$23.00.

The Growth of Islam among the Yoruba is a richly documented, pioneering study of a complex and hitherto relatively unexplored subject. Perhaps one of the greatest difficulties confronting any scholar who undertakes such an examination is that the Yoruba, although they share a generally common culture and language, are subdivided into numerous communities that differ considerably in social and political organization. Add to these differences the profound political upheavals that battered them

during the nineteenth century, as well as the increasingly persistent efforts of the Europeans to penetrate the region as merchants, missionaries, and finally as rulers, and one begins to perceive the daunting task that faced T. G. O. Gbadamosi as he sought to identify the various mechanisms that allowed and even encouraged the growth of Islam. The result, not surprisingly, tends toward a series of histories of Islam in individual communities such as Lagos, Abeokuta, Ibadan, Iwo, and Epe. Such "microstudies" would necessarily provide the basis for any general observations about the spread of Islam, but Gbadamosi has chosen to emphasize particular cases rather than broader, generalizable patterns.

Nonetheless, general themes do emerge, some of them consistent with the spread of Islam elsewhere in West Africa, others seemingly peculiar to the Yoruba. Islam was originally introduced from the north, a consequence of the long-distance trade routes and the importation of Muslim slaves. A significant Muslim immigrant group was composed of former slaves returning from Brazil and Sierra Leone. Precisely how the larger Muslim communities began to form around these small beginnings is never clearly stated, but by the last quarter of the century Muslims had attained positions of prominence, as military leaders and as titleholders, in many Yoruba states. Because titled offices in the Yoruba states required participation in non-Muslim rituals, Muslim titleholders were often considered by their coreligionists to be "tepid" in their beliefs. And although a learned coterie of educated Muslims had developed, whose purist views were reinforced by the activities of itinerant scholars from the north, compromises with traditional practices were tolerated, and the established political order was not directly challenged by the new Muslim communities.

This situation is very similar to that which obtained in other parts of West Africa where large segments of the population had become Muslim but where no move to establish an Islamic theocracy developed. But Gbadamosi has produced one of the few examples of how this Islamization process was aided by the non-Muslims themselves. He characterizes the Yorubas as religiously tolerant; of course there were conflicts and animosities between Muslims and traditionalists, but these tended to be directed against Islam as a political force (as in Ilorin, which falls outside the scope of this book) rather than as a set of religious practices and beliefs. Indeed, by the 1860s many people were becoming Muslim as a result of indications given them from Ifa divination. The traditional system could therefore contribute actively to the growth of Islam, and no evidence exists to suggest that the Ifa priests were ever challenged about their findings in this regard. This process tells us as much about the

traditional Yoruba religion as it does about the growth of Islam and suggests the necessity for a deeper understanding of indigenous African religion as well as sociopolitical systems in the study of religious change. Perhaps Gbadamosi's study will encourage others to follow his lead by further exploring these kinds of questions.

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MARGUERITE YLVISAKER. *Lamu in the Nineteenth Century: Land, Trade, and Politics*. (African Research Studies, number 13.) Boston: African Center, Boston University. 1979. Pp. ix, 222. \$9.00.

The towns of the East African coast, with their Islamic culture and historical ties of migration and trade to Arabia and the Persian Gulf, were treated by generations of historians as alien outposts of a distant civilization, divorced from the life of the rural mainland. Marguerite Ylvisaker is one of a generation of more recent African historians who have shown convincingly that the society, culture, politics, trade, and productive economy of the towns were rooted in the soil of the African mainland. She explains that this history of the northern coast of what is now Kenya "is written . . . if not with one's back to the sea . . . at least with one foot in the *mtama* [sorghum or millet] field." Whereas narrower works would concentrate only on the elites of the coastal towns, whose genealogies claim origins in the Islamic heartlands, Ylvisaker populates her work with the Oromo and Somali pastoralists of the dry interior, Bajun farmers of the wetter coastal belt, Pokomo agriculturalists of the Tana River Valley, and Boni hunters, in addition to the mainland farming colonies populated in the nineteenth century by escaped slaves.

Attention to the mainland enables the historian to explain long-misunderstood patterns of alliance between inland peoples and factions in the three main towns: Lamu, Pate, and Siyu. The history of the entire coast from the start of the Portuguese period is filled with mysterious marauding mainlanders who appear without reason to sack a coastal town, then just as quickly disappear. Ylvisaker shows, in an excellent, detailed reconstruction, how inlanders held a share of power within the island towns during the early years of the nineteenth century. Armed struggles between two towns would lead each to mobilize the forces of the interior as well as the coast. Somali leaders had long been present on the island of Siyu, sharing power with the ancient dynasty of the town. The leaders of the competing town of Pate found their allies among the non-Muslim Oromo pastoralists.

When the newly arrived Omani Arabs began their conquest of coastal towns in the nineteenth century, they exploited the factional divisions, took control of Lamu, and established large, slave-cultivated plantations on the mainland opposite. They, like the older rulers of the coast, were unable to colonize the mainland beyond their own plantations. The Omanis were forced to seek pastoralist allies, but even with this help were unable to remove the mainland colonies that their enemies now established within easy reach of the water's edge. The ruler of Pate, driven out by the Omanis, established a fortified town and made common cause against the Arabs with colonies of escaped plantation slaves and some Bajun.

Ylvisaker, who devotes a significant part of the book to the colonies of resistance, has a much sharper eye for political detail than for the moving forces of social tension. Even after the publication of this book, with its clear and precise narrative, an important story remains to be told: of the way escaped slaves made a new life for themselves in alliance with the old nobles who were now under attack by the plantation regime, of the way resisting nobles reconciled the contradictions between their roles as providers of refuge for escapees and as owners of slaves, of the economic bases of the resisting colonies, and of the perceived interests of the Bajun. This is to ask, however, for a different book in place of the honest and competent one being reviewed. Historians of East Africa must be grateful for the valuable reconstruction achieved in this work.

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MICHAEL BRATTON. *The Local Politics of Rural Development: Peasant and Party-State in Zambia*. Hanover, N.H.: University Press of New England. 1980. Pp. xii, 334. \$17.50.

After years of writing on national strategies and institutions, on powerful single-party states, on coups and instability, and on the apparent irrelevance of party structures, by Africa's second decade of independence scholars had rediscovered that African politics is largely a local-level phenomenon. Michael Bratton's recent book on Zambia fits solidly into the elaboration of that local-level perspective.

Bratton is concerned with politics and development in Kasama District in northeastern Zambia. He rejects two common claims: that the single party is a dominating, if not totalitarian, institution of centralized control and that politics in Africa is an institutionless arena, marked only by the ebbs and flows of shifting alliances and coalitions. Rather, he argues,

local, and therefore national, politics in Zambia can be understood in terms of an identifiable and comprehensible mix of self-interest, elite circulation, strategic coalitions, conflicts over resources, and patronage networks, all embedded in party and state institutions.

Bratton argues that in Zambia the mass mobilization of the anticolonial struggle has been replaced by a politics of survival for the party-state. The distribution of centrally controlled resources is used to deflect and mute local discontent. What results is the emergence of patronage networks that permit the re-establishment of earlier local leadership arrangements and that effectively demobilize most of the peasantry. A political machine is constructed, in which leaders offer material rewards (both individual—access to credit—and collective—infrastructural development projects) in return for support, either a muting of criticism or votes. Even opponents of the party offer a set of alternative elites, not an alternative politics (for example, the mobilization of the peasantry as a class).

Bratton's work, based on field research in the mid-70s, is informed, perceptive, and sensitive. He uses well his access to local interactions and references to studies elsewhere to expand our understanding of Zambian politics, whereas most writing focuses on national, urban, and copper-related issues.

Yet it is frustrating that several important issues noted by Bratton are neither pursued fully nor integrated into the analysis. For example, rural development is a major theme, but the focus is on the process of decision making, with little attention given to the outcomes. We are left wondering about the impact on local lives of the new and tarred roads, the expanded schools and hospitals, and the decreased self-sufficiency in food production.

Another theme is the use of centrally controlled resources (largely the income generated by copper sales) to construct and maintain the local political machine. We are left wondering, however, about the claims of other authors that there is in fact a net outflow, not inflow, of capital from the local area. That is, control over agricultural organizations, manipulation of marketing arrangements, management of credit, and the expenditures by migrant laborers in the copper belt may all result in unfavorable terms of trade for the local area. If prices for imports into rural areas have risen more rapidly than prices paid to farmers for their harvests, then are not the peasants subsidizing urban life and industrial expansion? If so, then the local conflicts are not only over shares of central allocations (the most visible struggles) but also, and perhaps primarily, over the forms and extent of exploitation.

Also frustrating is the relative inattention to national and international contexts. The 1960s and

1970s saw settler rebellion in neighboring Zimbabwe (Rhodesia) and decolonization in Angola and Mozambique. Major new routes northward—road, rail, and pipeline—passed through the area Bratton studied. There was successful peasant mobilization not only all around Zambia but also within Zambia, where the major Angolan and Zimbabwean liberation movements experienced violent internal conflict. How were all of those manifested in local politics?

Bratton's work, then, is solid and important. It is an essential antidote to the national focus of earlier writing and a measure of the success of that antidote. Having recognized the importance of the local, we need now to reintegrate that into our understanding of the national and the global.

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JON M. BRIDGMAN. *The Revolt of the Hereros*. (Perspectives on Southern Africa, number 30.) Berkeley and Los Angeles: University of California Press. 1981. Pp. 184. \$14.95.

Between 1903 and 1907 a series of revolts against German rule occurred in the colony of Southwest Africa, desperate efforts on the part of African tribes to retain their land, their cattle, and their way of life. Although these revolts are usually regarded as minor episodes in imperial history, they were remarkably difficult for the German government to subdue, and they ended with thousands of deaths and the destruction of the social fabric of most of the tribes of the territory.

Jon M. Bridgman gives us a short introduction to the geography and history of Southwest Africa and discusses uprisings of the Bondelswarts and the Hottentots as well as the Hereros. German rule is presented as a classic colonial case: seizure of African land and cattle, introduction of German settlers, railway building, concessions, bribery, and a policy of divide and rule. The author examines in detail the major campaigns of the Herero and Hottentot wars. The Germans were initially quite unable to cope with African guerrilla warfare and were forced to bring in large numbers of troops and supplies, over a period of several years, before they achieved victory. They then pursued a policy of almost total extermination; the Hereros were forced into the Omaheke desert, and their numbers were reduced from over 80,000 people to 20,000 in two years' time. This military policy was endorsed by the kaiser, the Reichstag, the settlers, and, particularly to Bridgman's horror, the bureaucrats in Berlin. The militarism and racism of which the Germans were accused in World War I were certainly documented in their actions in Southwest Africa.

Bridgman builds his account on several well-known general accounts of Southwest African history, and it is useful to have some acquaintance with them before reading this work. He makes major use of the official German history of the wars and of German military memoirs. Despite his interest in the African side of the wars, there is very little except speculation about African motives and strategy. Since he tells us that there is adequate material for biographies of several of the major leaders, such as Samuel Maherero and Hendrik Witbooi, it is regrettable that he has not included more about them. His description remains surprisingly impersonal; there are frequent quotations that are simply ascribed to "an officer" and several indirect references to the testimony of "the missionary Kuhlmann," who visited the Herero camp. Surely we might be given more information than this.

The accounts of military campaigns are thorough but frequently confusing. In a description of the siege of Okahandja, for instance, it is stated on page 78 that Captain Franke and the Second Company arrived to relieve it on January 28, 1904; a second account on page 81 has Captain Franke and the Fourth Company arriving on January 27. Since he is writing for an American audience, it would also be useful to know why Bridgman refers throughout to the 1968 German edition of Helmut Bley, *Kolonialherrschaft und Sozialstruktur in Deutsch-Südwestafrika*, which has been available in English since 1971.

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ASIA AND THE EAST

BRIAN E. MCKNIGHT. *The Quality of Mercy: Amnesties and Traditional Chinese Justice*. Honolulu: University Press of Hawaii. 1981. Pp. xii, 172. \$15.00.

The use of amnesties and general acts of grace as an instrument of political expediency was not unique to China, but, as Brian E. McKnight points out, no society has used them more extensively than the Chinese. Some of these acts were concerned solely with reducing or forgiving the penalties of criminals, but many others, in addition to their provisions for criminals, also provided benefits such as tax remission or gifts of money or rank to a wide variety of social groups. Beginning with the unification of the empire in the third century B.C., general amnesties and acts of grace were granted with considerable frequency, and by T'ang and Sung times they occurred about every two years.

McKnight provides some reasons for this exceptional use of amnesties in China. Central to the

Chinese political ideology was the concept of the emperor as one who ruled by virtue of a mandate from heaven, a mandate that could be withdrawn should the emperor fail in his primary task of providing for the common good. One of the most obvious signs of such failure was the presence of injustice, especially that caused by the emperor or his officials. Legal codes, also sanctified by tradition, were extremely rigid. This resulted in the need for various means to reduce the evil consequences that were bound to follow wrong or excessive punishment. Amnesties were especially appropriate for this purpose because they allowed for the proper execution of the law while at the same time providing an opportunity for the correction of any wrongs that might have occurred in the judicial system. Amnesties were further bolstered by the belief that the emperor, among his special powers, had the capacity to cleanse individuals of their taint and reintegrate them into the social body.

On a more mundane level, newly enthroned emperors would seek to reassure potential enemies, and those who were shaky in their positions would want to increase their popularity in any way they could. McKnight believes, however, that the true key to understanding the extensive use of amnesty lies in the fact that the traditional Chinese government was simply too small to govern in an intensive manner. In the Southern Sung, for example, some eight hundred judges of courts of the first instance were bound in law to handle the criminal problems (and some civil problems) arising among fifty to sixty million people. McKnight therefore concludes: "Given the ruling group's commitment to the proposition that all justice should be state justice, so long as the capacity of the police apparatus to capture criminals exceeded the capacity of judicial apparatus to process cases and control convicts, the authorities would be faced with a perennial overload. . . . The main function of the amnesty system was to relieve this pressure" (p. 120).

Following the Southern Sung, amnesties and acts of grace continued to be promulgated at a rate unprecedented in other societies, but there was a marked decline in their frequency. This seems to have been caused by several factors including the fact that the Liao, Chin, and Yuan were dynasties of foreign conquest whose acceptance of traditional Chinese ideology was tempered by practical considerations of military rule. They were less favorably disposed to using amnesties as a means of solving the problem of bureaucratic overload. Instead, the late imperial state seems to have been less concerned with defending its prerogative as the dispenser of all justice and thus sought a solution to the problem of overload by pushing more of the responsibility for handling disputes and minor criminal cases onto local elites.

McKnight has presented us with a fascinating study of a heretofore little understood institution and, in doing so, has opened a whole range of new questions concerning the changing role of law and bureaucracy in Chinese judicial history. One comes away from reading this book with a feeling that, while one small piece of a puzzle has been neatly explicated, that piece in itself will remain little more than a curiosity until put together with others in a much broader context. For the moment, however, we can only be thankful to McKnight for this valuable and eminently readable monograph.

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DAVID G. JOHNSON. *The Medieval Chinese Oligarchy*. (Westview Special Studies on China and East Asia or Studies of the East Asian Institute, Columbia University.) Boulder, Colo.: Westview Press. 1977. Pp. xiii, 281. \$24.50.

In this volume, David G. Johnson analyzes the formation of social elites in China during a time span of more than six centuries (A.D. 265–906). He uses several lists of eminent clans that survive from the Chin through the T'ang dynasties along with data gathered by Chinese scholars in Taiwan and Hong Kong, especially those by Mao Han-Kuang, to identify the social background of ranking officials in the various dynasties under study. Johnson notes that until the end of the sixth century 75 percent of the highest ranking officials in the various dynasties belonged to such eminent clans. In the following Sui and T'ang periods, the eminent clans provided 60 percent of the ranking officials. Johnson thus rightfully defines the "medieval ruling class" as an oligarchy. In this medieval period, the elites' status was ascriptive rather than achieved, but a change took place when a new society emerged in the Sung dynasty on the basis of a reshaping of the social structure that began in the chaos of the latter part of the T'ang era.

The author meticulously examines various definitions of the social elite (*shih*), a term that is derived from the lower echelon of the ancient feudal class structure and that in later days developed into what are often called literati-bureaucrats. In order to define the features of a great clan, much space is devoted to an analysis of the contents of several T'ang clan lists. Johnson's summary of the major theories concerning medieval China is very helpful. By noting that the most important social changes occurred in the late T'ang and succeeding Five Dynasties periods, he endorses Naito's system of periodization of Chinese history rather than Chen Yingko's. Chen puts the watershed in the seventh century, not in the tenth century, the date preferred by Naito and Johnson.

This is a very useful work, competently constructed on valid data. Although the scholarship is indeed laudable, the author cuts off his study just as he begins to analyze the conditions that seem to be responsible for the disintegration of the oligarchy. We can only wish that the author would give us an analysis of factors leading to the long sustenance and final disintegration of the oligarchy.

The basic pattern of the evolution of such an oligarchy in China in fact can be worked out using Eisenstadt's conceptual scheme of political systems. The political orientation of Chinese elites developed because of the destruction of the private commercial and manufacturing sectors of the Chinese economy by the great expansion of political authority during the Han dynasty. The development of an agrarian economy also explains the close association between elite status and the landed proprietary. Thus, the oligarchy fell as soon as the elites lost their landed home base in the northeastern provinces and became parasitic of the imperial authority and its military supporters.

Since Johnson will remain in the field as a serious contributor, we can look forward to his sequel volumes in which more profound questions on the formation and function of the social system in medieval China can be posed and answered.

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RAY HUANG. *1587, A Year of No Significance: The Ming Dynasty in Decline*. New Haven: Yale University Press. 1981. Pp. xiii, 278. \$19.95.

The focal point for this latest work by Ray Huang is the significance of the lack of significance of 1587, the Year of the Pig. Huang has focused his study upon several of the major, if not controversial, figures of the later Ming period: the Wan-li emperor cloistered as a prisoner of the system that permitted him to reign for forty-eight years; the grand secretaries Chang Chü-cheng and Shen Shih-hsing, both disgraced for their different efforts and styles; the censor in chief Hai Jui; the regional commander Ch'i Chi-kuang, and finally the enigmatic and often controversial thinker Li Chih. Huang uses each of these figures to demonstrate the basic inadequacy of the Ming governmental systems. From his point of view each figure demonstrates the failure of the Ming government to function in critical areas: the bureaucratic management, judiciary system, military defensive system, and the right of the individual to hold his own philosophical point of view outside the officially ordained orthodoxy of the Ch'eng-Chu school of Neo-Confucianism.

The "fall guy" in this approach seems once again

to be the Confucian tradition itself, and, at this point, considering the amount of research that now exists on Ming schools of Neo-Confucianism, this is definitely the most debatable part of Huang's model. Huang is arguing that China's reason for failure in the broadest sense (implicitly suggesting the late Ch'ing failure to adapt to "modernity") was its inability to evolve a governmental system free of axiological deliberation. A legal problem could not be separated from a moral problem, and this inevitably spelled the failure of the development of an adequate judiciary system. Huang argues, for example, that the succession problem for the Wan-li emperor became a moral issue simply because there was no other model to revert to, and, as a result, it virtually nullified any and all attempts to present appropriate legal arguments. The grand secretaries Chang and Shen are both victims of the same moral retribution and exemplify the failure of the development of adequate systems of bureaucratic management. The same argument is made for the office of the censor as well as for the inadequacy of the governmental response to Ch'i Chi-kuang's request for a rebuilding of military defenses.

Perhaps Huang's most controversial choice of individuals to demonstrate the destruction of the individual as well as the effective channeling of creative input in a thoroughly recalcitrant system is Li Chih. Li Chih is certainly an interesting example, and a good deal of work now exists dealing with him. It is debatable, however, whether Li Chih is as symptomatic of the Ming intellectual as Huang seems to assume. The issues that Li Chih opposes in Ch'eng-Chu orthodoxy are reflected by others as well, but it certainly does not lead others to the same conclusion of self-destruction as the reaction of Li Chih would suggest if he is indeed the paradigm of the Ming thinker. For Huang the problem for Li Chih lies implicitly within the Neo-Confucian system of thought itself. In this as well as in the other figures chosen it would seem that, in light of the now significant research in Ming thought, it might be possible to see the Confucian tradition trying to mold the moribundity of the system rather than remaining a prime cause of such lack of significance.

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PAUL S. ROPP. *Dissent in Early Modern China: Ju-lin wai-shih and Ch'ing Social Criticism*. (Michigan Studies on China.) Ann Arbor: University of Michigan Press. 1981. Pp. x, 356. \$24.00.

This useful and insightful book is the most comprehensive study available on the eighteenth-century

Chinese novel, *Ju-lin wai-shih* (*The Scholars*), one of the world's great sociopolitical satires. Paul S. Ropp places the novel and its author, Wu Ching-tzu (1701–54), in a series of illuminating contexts, including the changing socioeconomic structure of Ming and Ch'ing China and the body of social and political criticism that dates from the sixteenth century. The early chapters of the study are informative, despite their often perfunctory, schematic quality. They deal with the novel's three major targets. First, the civil service examination system is faulted for deadening formalism, the narrow philosophical basis of its questions, administrative corruption, and (most interestingly) the subversive effects that the system had on Confucian family loyalties. Second, the novel criticizes the subordinate role of women (under such topics as footbinding, education, concubinage, widow suicide). And last, the novel incorporates "skeptical" and "naturalistic" attitudes toward the supernatural—in both popular and elite culture—expressed in astrology, geomancy, or Confucian temple building and sacrifices. In its denial of the cosmic mechanism of moral retribution, the novel "marks a dramatic step in the secularization of popular fiction and a distinct crack (however small) in one of the foundation stones of the Confucian state and world view" (p. 109).

Ropp's important argument emerges clearly, especially in the last two chapters, where he seems to be more comfortable with a speculative and analytic mode. It is that the "dissent" of the novel—for all its newness of form—is a familiar kind, with rich precedents in the perennial dissent of philosophic Taoism, in the ancient bureaucratic tactic of withdrawal and "self cultivation," and in the much closer sixteenth- and seventeenth-century patterns of political protest, individualism, and egalitarianism. Addressing recent Chinese Marxist evaluations of *The Scholars*, Ropp argues that Wu Ching-tzu was perhaps a "feudal subversive" but hardly a "feudal reformer" and that Wu's ultimate response to the foibles, corruption, and "conservative academicism" around him was neither despair nor activism, but rather pessimistic resignation, passivity, and change in personal attitude.

The potentials of Ropp's analyses are not always fulfilled, for he often stops short in his explorations of the most important and intriguing questions he raises (such as the nature of the Chinese elite's embourgeoisement and its effect on literature and social criticism). And the study would have profited greatly had the imaginative and provocative allusions to *Quixote* and references to the work of Trilling and Ian Watt been more fully pursued earlier in the book. Nevertheless, this is a very readable and reliable point of departure for the generalist wanting an introduction to *The Scholars*

and its age and to traditions of dissent that we are gradually coming to recognize as basic to the culture of China's old regime before the advent of Western influence.

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SHERMAN COCHRAN. *Big Business in China: Sino-Foreign Rivalry in the Cigarette Industry, 1890–1930*. (Harvard Studies in Business History, number 33.) Cambridge: Harvard University Press. 1980. Pp. x, 332. \$20.00.

Based on a detailed study of the published documents from the Nanyang Brothers Tobacco Company, Sherman Cochran's book also includes the story of the British-American Tobacco Company (BAT) and of the two firms' aggressive competition for the Chinese cigarette market. In order to clarify such popular concepts as imperialistic exploitation, economic nationalism, and entrepreneurial innovation, the findings on the two multinational enterprises are tested against various definitions of these concepts.

Despite the book's subtitle, which highlights the interesting Sino-foreign rivalry between the Nanyang Company and the BAT, the two giants in the Asian cigarette market may nevertheless be regarded as two competing multinationals, tapping China's inexhaustible reservoir of low-waged, disciplinable labor forces, penetrating its vast inland markets, and grafting their operations onto the existing intra- and inter-regional networks for trade and commerce. From the angle that such multinationals could be owned by non-Western capitalists (for example, the pre-World War II Mitsui and Mitsubishi corporations), our vision of a clear-cut dichotomy—native Chinese versus Western aliens—becomes increasingly blurred, especially as we approach the actual operations of the Nanyang Company and the BAT. Although the author eloquently points out that native workers, agents, and raw materials from China played a pivotal role in the BAT's growth, the book also sheds light on the international background of the rise of the founder of the Nanyang Company, Chien Chao-nan, a Cantonese emigrant and naturalized Japanese citizen, who launched his business in Japan, Hong Kong, and Southeast Asia and who also acquired the initial capital, technology, and managerial know-how for Nanyang outside of China. Both the BAT and the Nanyang Company took advantage of the prestige and privileges enjoyed by foreign corporations in the late Ch'ing period. With the rising nationalism in twentieth-

century China, both the BAT and the Nanyang Company played the politics of nationalism, calling each other running-dogs of imperialism, and both were driven out of China in 1952.

The book, therefore, has a much broader relevance than most traditional business histories. Guiding his readers through the jungle of Chinese politics, for instance, the author's skillful narrative touches on prominent political figures, financiers, intellectuals, scholars, and journalists. The rich findings of this study, however, deserve a firmer setting in economic and business geography. The bulk of BAT cargoes from Shanghai to Tientsin, for instance, were *not* "shipped up the Grand Canal" (pp. 18–19). Maritime shipping was cheaper and faster than the junk freighters along the canal, many sections of which were hardly navigable by them. Another example of the importance of geographical concepts is Shanghai's business community, which was structured around the major regional groupings of powerful merchants. The dominant Cantonese group in Shanghai loomed large in the background of the Nanyang Company leaders' efforts to tap the financial pool in Shanghai.

Although a finer grasp of the Chinese social context is needed, the empirical details, based on the author's patient combing of archival materials, will insure the lasting value of the book as a major English-language reference on the subject. The Nanyang Company's family-dominated operations, for instance, which led the author to suggest the criticism of nepotism (p. 215), offer a gold mine for students of Maurice Freedman's model of Chinese corporate lineages. The Nanyang management usually discouraged matrilineal relatives from holding responsible positions. As members of the patrilineal Chien clan, the Nanyang brothers and cousins had their legitimate share of duties and privileges.

The two founding brothers, Chao-nan and Yü-chieh, were obliged to grant a minimum amount of 8 percent of Nanyang stock shares to each of the sublineage divisions represented by the sons of their deceased younger brother and Ying-fu, their youngest brother. After Chao-nan's kinsmen attempted to boycott some of his policies, he felt the need for legal and financial arrangements to guarantee his absolute control of the company. Still, the lineage tradition obliged him to allow the junior lineage members a minimum amount of stock shares.

Greater power in the management of the Nanyang Company meant greater responsibility in the Chien family. Handling Ying-fu's mismanagement of the company funds a few years after Chao-nan's death, Yü-chieh, now chairman of the board, turned to Chao-nan's widow for help. It was these two divisions, headed by Chao-nan's widow and by Yu-chieh—jointly controlling over 75 percent of Nanyang stocks—that took over the responsibility

for clearing Ying-fu's debts. In the context of lineage traditions, such intrafamilial dealings bring into relief the organizational matrix of one of the earliest industrial corporations in China.

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ROBERTA ALLBERT DAYER. *Bankers and Diplomats in China, 1917–1925: The Anglo-American Relationship*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. xxvii, 295. \$30.00.

From the collapse of the Yuan Shih-k'ai government in 1916 until the Kuomintang's capture of Peking in 1928, political authority in China virtually disintegrated. Chinese historians, both Communist and non-Communist, have long placed much of the blame for the chaos of the warlord years on the imperialist powers' activities in China. Foreigners' control of the major sources of Peking's revenue (customs and the salt tax), their privileged status in the treaty ports, and their incessant meddling in Chinese internal affairs precluded the establishment of an effective national government, contend most Chinese historians. Among many Western historians of China, however, there has been a strong tendency to downplay the effects of imperialist intervention in China, which is often dismissed as peripheral to major changes in China or conversely is credited with stimulating modern economic growth.

Roberta Allbert Dayer's new study, *Bankers and Diplomats in China, 1917–1925*, sharply challenges these Western viewpoints, arguing instead that despite pledges to restore China's sovereignty, both American and British governments and private authorities were eager to dominate the economic and political development of China and continually blocked Chinese efforts to initiate economic projects independent of foreign control. London and Washington promoted the banking consortium to control China's access to international borrowing, and both governments attempted to persuade China to arrange a large consortium loan to solidify foreign control in China, and not, incidentally, to ensure that China would have sufficient revenue to repay earlier foreign debts. The United States was most eager to gain control of the Chinese Eastern Railway, and, when the Lin-ch'eng kidnapping incident of May 1923 placed Peking in an embarrassing position, London saw this as an opportunity to force China to accept a unified railway system under foreign control. In sum, Dayer contends that, despite some Wilsonian rhetoric from Washington,

American and British policy was clearly inimical to the interests of Chinese nationalism.

Yet few of these schemes succeeded. The consortium was a failure, the Chinese Eastern Railway ultimately passed to the Japanese, and the unified railway scheme died quietly. Key to this failure was the rising hostility of Chinese—even warlords—to any extension of foreign control in China. Every initiative was matched by increased opposition. Equally important was the disunity of the four major powers in the East—the United States, Britain, Japan, and the Soviet Union. Relations between the first three and Russia were extremely hostile and Japan had serious disagreements with the United States and Great Britain. Dayer's book, as the subtitle indicates, focuses on the Anglo-American relationship. Her study is based on research in State Department and Foreign Office documents as well as the papers of major government and banking officials on both sides of the Atlantic. Dayer details a policy of cooperation on China between the two powers emerging during World War I (in part to block the Japanese) but ultimately fading during the 1924–25 period. Finally, Anglo-American actions in China were undermined by serious differences between government and banking interests. In particular the J. P. Morgan Company, which represented the United States in the consortium, often resisted State Department initiatives. Not only did the company maintain close ties to Britain and Japan, but it was also an extremely conservative investment company unwilling to take risks for political reasons.

Dayer's book admirably discusses the complex interaction of Anglo-American government and banking interests with regard to China but is disappointing in explaining the impact of the activities of these groups on China itself. She does not use Chinese sources, and a noticeable limitation of the study is the weak linkage between the story she tells and events in China. Finally, Dayer has a tendency to let her rhetoric run away from her evidence, particularly in her introduction. These criticisms, however, should not obscure the importance of this study. In particular, scholars in the People's Republic of China who are now re-establishing contacts with the West may find this work of special interest as it deals with subjects long discussed there but is based on materials unavailable in China.

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MARTIN COLLCUTT. *Five Mountains: The Rinzai Zen Monastic Institution in Medieval Japan*. (Harvard East Asian Monographs, number 85.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1981. Pp. xxi, 399.

The central focus of this book is the study of the historical and institutional aspects of the Gozan ("Five Mountains"), which is one—albeit the most influential—tradition of the Rinzai Zen in medieval Japan. The significance of the book lies in the deft way Martin Collcutt combines a number of historical strands that have not often been joined together. He recognizes the importance of viewing the checkered development of Zen in Japan against the background of its Chinese model. Leading Ch'an monasteries had developed a three-tiered and officially regulated hierarchy headed by the "Five Mountains": three temples in Hangchow and two in Mingchou.

The book is divided into two main parts. Part 1 traces the development of the Gozan institution; part 2 presents its structure. In part 1 the author discusses the pioneers of Japanese Zen and seeks to understand their motivation to study and their methods of appropriating Zen. He also looks into who were the pioneers' secular patrons and why these patrons supported them. A relative latecomer to the Japanese Buddhist scene, Zen needed financial support and protection against the hostilities of the older Buddhist institutions that held religious, cultural, economic, and political strength and that were respected and feared both by the imperial court in Kyoto and the warrior-regime (*bakufu*) in Kamakura. Zen also had to compete with the popularity of new Buddhist schools such as the Pure Land and Nichiren sects. Collcutt rightly emphasizes the crucial importance of the Chinese émigré monks welcomed by the warrior-rulers at Kamakura because they infused new spirit into the Japanese Zen movement but also because they bore Sung art, culture, and learning, including Neo-Confucianism. Thanks to the Chinese monks' help, the warrior-rulers at Kamakura felt released from an inferiority complex relative to the sophisticated courtiers in Kyoto. Significantly, Zen and other aspects of Sung culture were equally attractive to the imperial house and to the rulers of the Ashikaga *shōgunate* (1338–1573) who also supported Zen with mixed motivations. In the end, the Gozan system, as articulated in Japan, was a uniquely Japanese institution hammered out of Japanese historical and cultural situations. It is remarkable that Zen institutions, especially the major Gozan monasteries, which remained in one sense centers of Chinese religion, learning, and culture, exerted such profound influence on art, culture, economics, and learning in Japan even during the period of social and political disintegration in the fifteenth and sixteenth centuries.

Part 2 of the book deals with Zen monastic life and rules, the relation of the monastery to its subtemples, the monks and others who comprised the monastic community, and an analysis of the Zen monastic economy. Incorporating the findings of

current Japanese scholars, the author's careful research in these institutional aspects of Gozan temples amply justifies his contention that Zen Buddhism, especially the Gozan branches, was a vigorous and pervasive institution in medieval Japan (p. 291). This book should be read widely by students of history, Buddhism, and history of religions for its fascinating contents as well as its methodological astuteness.

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GEORGE ELISON and BARDWELL L. SMITH, editors.
Warlords, Artists, and Commoners: Japan in the Sixteenth Century. Honolulu: University Press of Hawaii. 1981. Pp. xvi, 356. \$20.00.

Except for the century since 1860, the sixteenth century was the most hectic in Japanese history. This volume attempts to capture the dynamism of that age and to illuminate its kaleidoscopic character by examining several aspects of culture, society, and politics. The last of the eleven essays, by Bardwell L. Smith, is an exhaustive, broadly inclusive bibliographical report on pertinent Western-language materials. In the absence of any adequate up-to-date annotated bibliographies on Japanese history in Western languages, this essay may prove to be the most valuable part of the book. The volume is nicely turned out and is graced with handsome figures and fold-out plates accompanying the essays on painting, dance, and music.

As an interpretive whole the volume is less successful. The essays reflect the different disciplinary orientations of the writers, and they vary in type of focus. Thus, for example, two essays examine particular individuals; another looks at a city to examine the nature and extent of medieval urban autonomy. One essay describes a famous set of long-lost paintings while others discuss the history of tea and its appurtenances from ancient China to early Tokugawa Japan, city attitudes toward the countryside and vice versa, and music high and low. Two essays examine general themes of political process in such dissimilar ways that the reader scarcely realizes they are examining the same age.

In a sense the diversity of the essays is a true reflection of the age as it appears in this volume. What emerges is a picture of an era of highly developed *de facto* individualism in which poets, artists, and tea masters could serve one ruler and then another, advancing their professional careers as best they could with no more sense of disloyalty than a modern job-hopping academic. Similarly, businessmen of Sakai could cooperate with whom-ever seemed likely to give them the best deal and still remain honest to their calling and community.

In his introduction to the volume, George Elison comments on the rich linkages binding the violently tumultuous world of rulers to the boldly creative world of artists. The extent of the association between rulers and artists is intriguing, seeming to contrast, for example, with Elizabethan England. A number of the essays illuminate the character and causes of the linkage and suggest how conscious self-interest related to shared notions of civility, authority, and social obligation to shape and sustain the ties among warlords, artists, and commoners. Readers will find in this volume a host of fascinating bits of historical information and insight that will serve as grist for interpreting one of Japan's most exciting epochs.

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ROLAND HUNT and JOHN HARRISON. *The District Officer in India, 1930-1947*. London: Scolar Press; distributed by Biblio Distribution Center, Totowa, N.J. 1980. Pp. xxxiii, 255. \$35.00.

This is an interesting, informative, and nostalgic survey of the life and work of district officers in India toward the end of British rule. It is based on the reminiscences of retired officials, and its two authors are well qualified to write it. Roland Hunt served in the Indian Civil Service and later as deputy high commissioner in Pakistan; John Harrison was a major in the Indian army and later taught Indian history at the School of Oriental and African Studies. They have struggled to make sense of the memories of seventy-two former district officers, and they have succeeded in conveying an impression of the wide variety of tasks that such officials had to perform as tax collectors, as magistrates, and, more generally, as local representatives of a remote and foreign government.

Hunt and Harrison see themselves (p. viii) as "setting out the evidence with the minimum of editorial explanation and comment." But such a bland statement invites questions. We are told that the project began in 1974, when three retired officials suggested it to the director of the India Office Library and Records, who thereupon sent a questionnaire to "a number" of their old colleagues. We are not told how that number was composed, although Hunt and Harrison describe their informants as "self-selected" and express regret that so few were of Indian birth. Perhaps even more important, we are not told what was in the questionnaire. Yet a knowledge of the questions asked, and indeed of their wording, would help us to assess the responses that they elicited: this omission is all the more serious when we are thinking of the recollection of events that occurred four or five decades ago.

Needless to say, the former district officers cherish warm memories of the days of their youth. They all seem to have been energetic, competent, and incorruptible, served by loyal subordinates and surrounded by admiring peasants. Their recollections of politicians, businessmen, and missionaries are apt to be less warm: Hunt and Harrison comment that officials tended to dislike the presence of rival claimants to authority and allegiance. In general, however, the comments made by Hunt and Harrison are more cautious, and at times they go to undue lengths to defend British administration from criticism. For example, after mentioning a complaint that courses on Indian arts, religious beliefs, and customs should have been provided as part of the training of young officials after their arrival in India, Hunt and Harrison assert that there were nevertheless senior officials who were prepared to discuss such things with their juniors. To substantiate this they mention one particular official who would discourse on "Indian independence" and Buddhism with a young Indian colleague. Buddhism, however, was hardly central to the concerns of most Indians in the 1930s.

Each chapter is followed by some references to further reading. But again the tone is deceptively bland. Books and articles are variously described as "sound," "readable," "distinctive," but no indication is given of the arguments propounded in them or of the controversies to which they have given rise. Even more disappointing is the absence of any reference to the extensive oral archive on the British in India, of which Harrison himself is the project director. Those who write history under official auspices may well feel subject to certain constraints. The endeavors of the India Office Library and Records in promoting the publication of these and other documents can never be forgotten. But there are dangers in assuming that historical understanding can be more readily facilitated by the publication of documents than by the asking of questions and the formulation of hypotheses.

KENNETH BALLHATCHET
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JUSTUS M. VAN DER KROEF. *Communism in South-East Asia*. Berkeley and Los Angeles: University of California Press. 1980. Pp. ix, 342. \$32.50.

This is a soundly designed, carefully written comparative study of the various Communist movements in Southeast Asia, their histories, and their present conditions. The key words here are "survey" and "comparative."

It is inevitable, I suppose, that a specialist like myself, and probably many lay readers as well, put down any comparative survey work with a certain

feeling of unfulfillment. That vast arc of lands and peoples that is Southeast Asia is both diverse and complex. It is the crossroads of four major world cultures, a place of enormous climatic and geographic range, and home for a bewildering mosaic of societies and races. As such, it is physically impossible to do justice to the region within the covers of a single book, much less in a truncated work such as a survey. Something of the same difficulty presents itself with respect to the comparative study. Certainly comparative study is a legitimate method, one that can often provide insight by singling out and highlighting similarities and differences, in this case those of the respective various Communist parties. Still, one is left with the uneasy feeling that, as in comparing apples and oranges, only a limited amount has been accomplished. None of this is meant as criticism but simply as an observation on the limitations inherent in a comparative work.

Any reservation I might have as a specialist, however, is offset by a much stronger feeling about work done in my field, Communism in Southeast Asia, namely that it is a subject woefully undertreated if not ignored. I therefore welcome heartily those contributions that do appear. Earlier comparative surveys on Southeast Asian Communism published during the past 25 years number perhaps half a dozen, all of them now at least a decade old: Virginia Thompson (1950), J. H. Brimmell (1959), Frank Trager (1959), A. Doak Barnett (1963), and Robert Scalapino (1969). In the 1970s there was no new full-length treatment of the subject that I am aware of; hence Justus M. Van der Kroef's book fills a void that has existed for too long. This is a matter of more than passing importance. Certainly it can be argued that one of the lessons of the Vietnam War was that mistakes such as that war are made far more likely when there is no continuing, systematic effort to keep abreast of developments by academics and others, no continuing flow of relevant research materials.

This book is a good introduction to the subject for newcomers and a valuable reference work (especially its biographical note) for those already in the field.

DOUGLAS PIKE
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GEOFFREY SHERINGTON. *Australia's Immigrants, 1788-1978*. (The Australian Experience, number 1.) Boston: George Allen and Unwin. 1980. Pp. 189. Cloth \$18.95, paper \$9.95.

Based on published documents and secondary studies, this little book retells a familiar story clearly and

fascinatingly. Four central questions are posed: Who were the immigrants? Why did they come? Where did they go? What did they do? Effective use is made of illustrations, maps, charts, and statistics; the text is supported by end notes; and there is a guide to further reading. Several themes help readers make sense of the data—the push-pull forces behind emigration, the role of family networks in providing information and support, government as recruiter and subsidizer of emigrants, and the policy debates over such issues as numbers, ethnic and religious origins, and occupational skills. Each of the five chapters is replete with lively answers to the author's questions, usually with anecdotal examples. Especially interesting is the treatment of the recent past, in which there have been significant differences in the earning power of immigrants from northern as compared to southern Europe.

The value of the book is limited, however, even for classroom purposes, by the superficiality of the author's purpose. His questions preclude analysis of the meaning of the immigrant experience for an understanding of Australian history. It would be helpful, for example, to know when and how (or even if ever) former convicts were restored to their civil and political rights. Similarly, we learn nothing of the legal status in the Australian colonies of free immigrants from the British Isles (were there variations from colony to colony? Did it matter whether one came from England, Ireland, or Scotland?), and there is not a single word about the status of emigrants from continental Europe, the United States, or even New Zealand. When did the concept of Australian citizenship emerge? What rules govern naturalization? Have they changed over time? Have some groups taken up citizenship more rapidly than others? Sherington makes much, as he should, of the magnitude of emigration from Catholic Ireland, but he says nothing consequential about what might be called "the Irish factor in Australian history."

These and similar questions go to the problem of explaining the emergence of an Australian identity. Sherington's "who, why, where, what" questions, although useful, leave unexplored the meanings and processes that really matter. One hopes the book will provoke students to insist that more penetrating questions be posed and answered.

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UNITED STATES

T. HARRY WILLIAMS. *The History of American Wars: From 1745 to 1918*. New York: Alfred A. Knopf. 1981. Pp. xviii, 435. \$20.00.

The late T. Harry Williams was a historian of keen intellect, exemplary assiduity, and uncommon originality. With the possible exception of such men as Samuel Eliot Morison and Louis Morton there are few instances among American historians of recent times of so happy a combination of natural ability and unrelenting application. *The History of American Wars* is an unfinished work cut short by the author's unexpected death in 1979. In a preface written by his widow we are informed that he left off with World War I and never completed the chapters he had hoped to write on World War II and on the Korean and Vietnam conflicts. But no matter. The historical profession and the general reading public will welcome this perceptive, well-written but unfootnoted synthesis of American wars from 1745 to 1918.

Williams was one of several contemporary authors who did much to revitalize the study of armed conflict and to make the subject informative, interesting, and relevant. On the eve of World War II military history, not entirely undeservedly, was in bad repute. Dominated by professional soldiers and buffs, the practitioners of the art generally emphasized the operational details of battles, leadership, and individual deeds of heroism. World War II changed all this. Under pressure of events some of the best students were drawn into the field, and as a result there was a heightened awareness of the importance of our military past and its relation to social, political, and diplomatic history. During and after the war Williams wrote excellent books on Lincoln and his generals and on the Confederate general, P. G. T. Beauregard. Another manifestation of the new trend was the publication in 1959 of a textbook, entitled *A History of the United States*, by T. Harry Williams, Richard Current, and Frank Freidel. The chapters and parts of chapters on military events, probably written mostly by Williams, were among the best to be found in any textbook then available. In 1960 Williams published *Americans at War*, a survey of some 140 pages, that used much of what appeared in the textbook but included other material as well. The present volume is a further revision and updating of *Americans at War*. Over the years Williams reviewed his own interpretation and those of other scholars and added material as the fruits of ongoing research became available. For example, *The History of American Wars* contains more information on the role of blacks in the armed forces than any of his previous works.

In this volume, as in his prize-winning biography of Huey Long, Williams develops a strong thematic line that goes straight to the heart of difficult and controversial matters. His organization is clear and his proportions good: each war is covered in one to four chapters and the years between wars are covered by "interlude" chapters that deal with econom-

ic and social changes as well as with technological developments as they affected the army, navy, and air force. Considerable emphasis is placed on command arrangements, the way the armed forces were organized and brought to bear against the enemy to achieve strategic goals. On so vast a subject covering so many years not every historian will agree with Williams's conclusions, but because of his wide reading and trenchant analysis his interpretations must be treated with respect. It is indeed sad to realize that there will be no more books or articles from the pen of this stimulating and provocative historian.

HARRY L. COLES

Ohio State University

TERRY L. ANDERSON and PETER J. HILL. *The Birth of a Transfer Society*. (Hoover Press Publication, number 229.) Stanford, Calif.: Hoover Institution Press. 1980. Pp. xv, 114. \$6.95.

"We live in a transfer society," declare economists Terry L. Anderson and Peter J. Hill (p. 12). A transfer society is one that is more concerned with shifting wealth among individuals or groups than with adding to its total stock of wealth. In their essay, Anderson and Hill claim to describe how this came about, emphasizing the decay since 1787 of the constitutional contract that defined and enforced private property rights.

Anderson and Hill argue that the origins of the transfer society lie in the Jacksonian era when people who were threatened by the industrial revolution turned to government to maintain their wealth positions. This defensive impulse, strengthened by two ideological shifts—a growing "faith in the goodness of man and human progress" and "the rise of egalitarianism"—established the transfer society after the Civil War (p. 53). It did so by lowering constitutional barriers to legislative transfers (that is, weakening of the restraints of the contract, commerce, and due process clauses and the broadening of the concept of the public interest). Anderson and Hill see the transfer society as firmly in place by 1915 and discount the New Deal as a contributor to its legal foundation.

Their essay has an overt political purpose: to shape ideas in order to produce "a constitutional revolution" that would limit transfers by "a set of fundamental, difficult to change rules." Because such constitutional revision could occur only within the politics responsible for the transfer state, the authors call for an "intellectual revolution" (p. 94). Some contemporary Marxists would praise their logic, arguing that resolution of the basic contradiction of liberal democracy in a capitalist society requires an ideological offensive to induce Americans to sacrifice their political rights to restore a

system more conducive to private wealth accumulation.

Without writing a rather different book, the authors seem unlikely to attract many to their side of the intellectual barricades. Their constitutional history is simplistic, lacks explanatory depth, and is largely derivative. In any case, it cannot carry the weight of their argument. They do not establish connections between constitutional change and the waste of resources through "zero-sum" games; at no point do they adequately define the basic trends in government growth, transfer payments, and waste. For evidence that transfers have grown since the Civil War, the authors merely allude to the expansion of the public sector, formal interest groups, and industry regulation. The authors acknowledge the benefits of transfer payments that can result from correcting illegitimate holdings of property rights and property and from achieving an optimal production of public goods. They do not, however, assess the extent to which programs of education, health, transportation, conservation, defense, civil rights, and even taxation may have had "positive externalities." Their disclaimers to the contrary, the essay is little more than a normative response to the central historical realities that the American economy (as well as the legal system) always has been a social instrument and that its performance always has been measured against values other than those of conventional productivity.

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JEFFREY G. WILLIAMSON and PETER H. LINDERT. *American Inequality: A Macroeconomic History*. (Institute for Research on Poverty Monograph Series.) New York: Academic Press. 1981. Pp. xx, 362. \$29.50.

This wide-ranging, imaginative, brash, and moderately persuasive work seeks to discern and explain broad changes in the trend pattern of inequality (of wealth, income, earnings of the free population of the U.S.) from the seventeenth century to the present. A polished culmination of widely circulated and widely discussed working papers and two published shorter versions, this important book draws on publications by Jeffrey G. Williamson on economic development and by Peter H. Lindert on demography. Sparkling prose enlivens the array of empirical data discerningly assembled chiefly from existing microstudies of probate, tax, census, wage and price records, in sixty-nine text tables, twenty-one graphs, and eleven appendixes. Compactness in data form, ideas, and arguments requires close attention. The Gini coefficient and the share of the top ten (or five or one) percent of families (or

income-receiving units or individuals) are used as measures, though the authors recognize the possibility of divergent results by other statistical measures or recipient units. The compendium of wealth and income studies alone, with accompanying evaluation, testing, and sometimes reconciling of inconsistent estimates, is a great contribution, ignoring minor quibbles that could be raised. The authors call for additional probate studies for periods of sparse data on wealth. They test and find not useful for trend explanation a number of intuitive and demographic variables.

The authors suggest that the course of American wealth (or income) inequality (for the nation as a whole) was essentially flat through the colonial period up to 1774 and has been flat since the 1950s at somewhere near the same level. For the intervening period of industrialization, from the early nineteenth to mid-twentieth century, earnings and income (possibly also wealth) inequality increased dramatically to 1860, probably continued more slowly and somewhat irregularly to a maximum around 1929, then turned down. For this whole period, they see inequality as essentially following Simon Kuznets's inverted "U" pattern, with slight bobbles for lesser peaks reached around 1860 and perhaps again in 1914, followed by a few wartime years (Civil War decade, World War I) of income leveling. The sustained downturn in inequality (income previously documented by Simon Kuznets, wealth by Robert J. Lampman) came from 1929 to the mid-1950s. During and since World War II, however, no clear direction of change in wealth or income distribution occurred.

The authors discuss the growth-equity conflict and claim to show that the Kuznets curve is not an inexorable, universal relation, delineating the time-path of income distribution for industrializing countries, past and present. They find no necessary trade-off between growth and equity linked to the alleged dependence of more rapid growth on higher rates of savings and capital accumulation. Rather, both growth and inequality may be outcomes of other multirelated causes. Their general equilibrium model is designed to show that the Kuznets curve for the U.S. was a side effect of: (1) unbalanced technical growth, with higher productivity increase in the sectors where technology was allegedly unskilled-labor saving and machine- and skilled-labor using, (2) rapid nineteenth-century growth rates of the unskilled labor force from high birth rates prior to 1840 and high immigration subsequently, (3) disproportionate output growth of sectors with rapid technical change, induced partly by demand ("Engel curve" effects—that is, high income elasticity of demand for consumers' durable goods and skill-intensive services) and partly by falling relative prices of capital goods that further

promoted their substitution for "raw" labor. After World War I, intersectoral growth rates in factor productivity became more balanced, and labor force growth rates declined, thus removing two major forces driving up inequality. Interoccupational wage differentials began to narrow after 1929, bringing on the equalization trend until 1950. Since then income distribution has stabilized, along with interoccupational wage differentials.

The fascinating and ingenious argument and model will not find complete acceptance, but they brilliantly exploit vast literatures and open profitable discussion. Application of a general equilibrium model to historical data may encourage others to use this way out of simplistic efforts to determine historical causes. The inequality trends do not accord with Lee Soltow's interpretation of no change in long-term wealth inequality, and the 1774 to 1860 increase is treated as greater than I see it. Robert E. Gallman, commenting on an earlier version (*Studies in Income and Wealth*, 46 [1980]) was generally favorable but questioned (p. 134) whether the authors were premature in declaring a distributional revolution to 1860. He also gave (p. 135) several reasons for not accepting for the first half of the nineteenth century the evidence for unbalanced technical change or for decline in the relative price of capital and was astonished at the limited attention to treatment of slavery. The argument and model leave many questions: What of economies of scale? Are American earnings data relevant to Third World countries? Are urban earnings overemphasized for dates when urban population was a small fraction of the total? Should wealth inequality be expected to move like income inequality?

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DAVID B. QUINN, editor. *New American World: A Documentary History of North America to 1612*. In five volumes. New York: Arno Press. 1979. Pp. lxxxviii, 486; xxiv, 594; xxvi, 494; xxvi, 454; xxvi, 572. \$350.00 the set.

The main value of this five-volume set for scholars is the areal organization of the surviving documentary evidence of North American history before 1612. David B. Quinn, the principal editor of the set, has made distinguished contributions to the historical scholarship of the period and brings an authority based on actual use of the primary materials that is probably unrivaled by any other living scholar. From the vast array of documents about European overseas discovery and exploration during the fourteenth to early sixteenth centuries, Quinn and his assistants included in this collection only materials

about North America. This focus produces a copious set that is extremely convenient for scholarly use because it brings together in a single collection items that in original form are dispersed in numerous libraries and archives and in printed form are scattered in hundreds of volumes. The convenience for scholars is the unquestioned value of the collection, which contains two types of documents: written materials and maps. The inclusion of maps and charts at the end of each volume is particularly laudatory because they are primary artifacts too often neglected by historians. The subdivisions of the set are both topical and regional with the coverage for each section reflecting the vicissitudes of survival.

For the most part Quinn offers the written documents without editorial commentary or interpretative annotation, although a brief introduction to each volume provides something of the general historical context for the materials in the book. To some minds the omission of commentary and interpretative discourse allows an unbiased approach to the documents, but it does mean that many passages in the often ambiguous texts will be interpretable only by a limited number of scholars who possess detailed knowledge of place-name and locational identifications and of other elements in the historical context. Quinn is not, however, consistent in his use of the format. The map notes contain interpretative comments that are not identified as such. For example, the identity of the coastal feature labeled *Cabo de Arenas* on early sixteenth-century Iberian cartography is controversial, yet Quinn without qualification states that the land form is Cape Henry (vol. 1, p. 483). This example is not the exception. If editorial interpretations are to be offered, they should be identified as such, and a bibliography should be provided for readers to investigate the controversy. Whether or not the interpretative commentary or annotative approach should be used with documentary material is a personal decision. I consider the method to be an informative and beguiling means to resolve the ambiguities inherent in these early documents and thus regret the omission from this collection.

In the case of the maps and the charts, a more regrettable shortcoming is the lamentable size of the reproductions. Most are so small that the print is illegible. Most of the maps and the charts have been reproduced elsewhere and in a quality of reproduction that is superior to these volumes.

If this collection contained a significant number of important documents that were receiving their first modern printing, the scholarly importance of the set could be defended on grounds other than regional comprehensiveness and areal organization. But most of the material in this collection was previously published in various documentary com-

pilations and is reprinted from them. An occasional new translation relieves the exact duplication, but some items in this collection are being reprinted for the third and fourth times. The collection is thus clearly derivative: old wine in a new flask. Is such duplication necessary? Yes, if the point be a comprehensive areal collection, but what about other criteria for inclusion in this collection? Quinn is more often than not silent or vague about the criteria for extracting selections from the older compilations. The publication history of some documents leads to some raised eyebrows about the standards and the importance of the chosen selections. A particularly unfortunate example involves the "Relation of Alva Núñez Cabeza de Vaca." The editors state that they use the 1906 version published by F. W. Hodge and T. H. Lewis, although a new version from 1972 is "an improved text." Why was not the latter used here? How many other selections are likewise not the best available modern versions?

Because so much of the Quinn collection is directly derivative from accessible published sources, a comparison of texts is a valid means to assess whether the new collection offers any advantage over its precursors. I compared the maps and the charts with a variety of other facsimiles and concluded that they were more legible reproductions than the ones in this collection. On the other hand, they lacked the advantage of juxtaposition with textual materials that the Quinn collection affords. Because the textual materials come from such a large number of publications, a complete comparison was impossible and, if executed, could not be reported in a brief review. So for a detailed comparison of earlier printings and the present derivative text, I selected "The Cabot Voyages and Bristol Discovery under Henry VII," edited by J. A. Williamson (1962). In volume 1 (p. 91), Quinn reprints a selection by William Worcestre from the Williamson collection. Quinn erroneously cites the page numbers in Williamson as 197-98. The correct page numbers in Williamson are 187-88 on which he gives both a transcript of the original manuscript and an English translation of it. On page 92 of the same volume, Quinn presents a brief selection by Robert Thorne. This time the page number for the passage in Williamson is correct. But Williamson rendered the passage with editorial insertions indicated by brackets; the brackets, but not their contents, are omitted by Quinn. My conclusion is that Williamson brought me closer to the originals than did Quinn—and, after all, bringing a researcher as close as possible to the primary evidence is a major purpose for the publication of documentary collections.

The regional and topical comprehensiveness of the Quinn collection makes it a valuable, if expensive, addition to the literature on European penetra-

tion of North America. As a documentary collection, the set is flawed and supplements rather than supersedes its precursors.

DOUGLAS R. MCMANIS

American Geographical Society

LYLE KOEHLER. *A Search for Power: The "Weaker Sex" in Seventeenth-Century New England*. Urbana: University of Illinois Press. 1980. Pp. viii, 561. \$25.00

A growing number of studies reveal that the farther south of New England colonial women lived, the greater their legal rights and ability to influence their individual destinies became. Thus Lyle Koehler wrestles with an important and difficult topic. Some difficulties stem from the lack of letters, journals, and diaries by seventeenth-century New England women. Town, court, and colonial records and other materials only partially compensate for this. Koehler presents interesting theories based on modern psychological and sociological studies, but often his own evidence damages or destroys them. He describes the cultural stereotype for women and the world view of early Puritans, analyzes female variations from and adjustments to the stereotype, and tries to demonstrate that Puritanism did not liberate the "weaker sex." In the process he provides interesting information about female criminals, innkeepers, widows, and others whom he claims provided an "unwomanly" model. This model enabled "secular" women of the 1690s to adopt an assertive lifestyle without relying on earlier religious justifications such as Antinomianism or Quakerism. He tries unsuccessfully to refute the contention that Puritan women were more liberated than those of later eras.

The author is annoyingly present-minded, and nineteenth- and late twentieth-century values impregnate his study. Several statements illustrate an inability to grasp the texture of seventeenth-century New England. For example, Koehler repeatedly refers to "Puritan ladies" and "proper ladies." Furthermore, although ladies in nineteenth-century cities often felt helpless and extraneous, Puritan women had a significant role in the most important social and economic unit of society. His emphasis on individualism and his claim that by the 1690s deference and the patriarchal family had lost their force, runs counter to persuasive evidence amassed by Michael Zuckerman, Richard Bushman, Philip Grevin, Kenneth Lockridge, and others. Many New Englanders, especially women, were illiterate, but strictures on the relative lack of institutional female education ignore Bernard Bailyn's insightful *Education in the Forming of American Society* and fail to follow the implications of Koehler's own evidence, which reveals that education for many women included reading, writing, ciphering, and accounting.

As a disciple of Nathaniel Hawthorne, Koehler depicts joyless Puritans, anxious about sex and using love to institutionalize the oppression of wives. At times he conveys confusing views regarding Puritans and sex, for within a few pages he claims that they thought sexual intercourse "a base & contemptible thing" and that they affirmed that women ought to enjoy sex.

Unlike Edmund Morgan and John Demos who explain that Puritans worked hard to keep families harmonious because they were so vital to the Puritan way of life, Koehler revels in stories of conflict. He attributes marital tensions to the Puritan Weltanschauung and explains that when a culture emphasizes female powerlessness and male dominance, wife abuse becomes fairly common. But he undercuts himself with zealous quantification; 128 cases of wife abuse in all New England during 70 years seems rather rare. Koehler asserts that pressure to marry young "often" led to bad marriages, but he can discover only 300 cases of adultery in the entire century—a number he decides makes it a "most common offense."

Koehler asserts that because Puritanism was so repressive and made women feel so powerless, it caused reactions associated with unhappy women in the nineteenth century. Hysterical fits were a classic Victorian symptom, and Koehler discovered four "epidemics." But the first three involved a total of only fifteen people (four of them siblings)! And all occurred while Puritanism was fading. A dozen cases of languishing hardly make that malady "not uncommon," and only twenty-one suicides by women in all New England before 1702 hardly validates his thesis. Similarly, his claim that during the 1690s many women were less willing to marry or play a maternal role is backed by inadequate evidence. A dozen pages of documents about Connecticut and New Hampshire underpin the first assertion, and twelve women (eight of them single) accused of infanticide support the second.

After a good chapter on the Rhode Island alternative comes a somewhat confused analysis of the Salem witch trials. Why is a movement against authority figures in a male-dominated society directed mostly against women whom Koehler describes as frequently eccentric or nontraditional? And something is amiss when on the same page the author identifies females as authority figures and "those perpetual children" (p. 395).

The book rests on extensive research and is well organized. Koehler often makes effective use of vivid quotations and examples. At times, however, numerous speculative sentences with their "perhaps" and "may haves" become annoying. And some sentences are cumbersome, such as the one that begins "the social atmosphere legitimating and intensifying norm slippage . . ." (p. 345). When

Koehler wrote this study he lamented that "many unsubstantiated interpretations abound in the available literature" (p. 445). Regrettably such interpretations abound in his own book.

RANDOLPH SHIPLEY KLEIN
American Philosophical Society

PATRICIA U. BONOMI, editor. *Party and Political Opposition in Revolutionary America*. Tarrytown, N.Y.: Sleepy Hollow Press. 1980. Pp. xi, 157. \$17.50.

The nine essays in this collection are revised and expanded versions of papers presented at a 1975 "Conference on Party and Faction in Revolutionary America" sponsored by Sleepy Hollow Restorations. After a brief but informative introduction by Patricia U. Bonomi and J. G. A. Pocock's dinner address, the bulk of the volume is given to four papers followed by comments.

Pocock's "Civil Wars, Revolutions, and Political Parties" is largely historiographical. He notes that his generation was brought up on "Namier-and-branch-water," that is, "there were no parties but only connections" (p. 1). Current thinking is dominated by the "Bailyn revolution," the study of the transatlantic migration of ideologies that were, according to Pocock, "taken infinitely more seriously in the colonies than in the country of their origin" (p. 7). He concludes that English and American parties differed because the colonies, being neither shires nor nations, could not duplicate the English scene. Following independence there was no polarization of principles like that of Whigs and Tories in England because "the history of Loyalism is the history of Canada," and the Federalist-Republican battles were over "differing interpretations of what the Revolution had been" (p. 11).

No sense of conflict pervades either Stephen Botein's "Religion and Politics in Revolutionary New England: Natural Rights Reconsidered" or Stanley N. Katz's commentary, "The Legal and Religious Context of Natural Rights Theory." The gurus of revolutionary New England turn out to be not British radicals but home-grown Calvinist clergymen. Parties go almost unmentioned, and the Revolution turns into a contract dispute.

Marc Egnal, on the other hand, in "The Pattern of Factional Development in Pennsylvania, New York, and Massachusetts, 1682-1776," finds parties were long-lived, coherent, and based on two clear issues: territorial expansion and imperial regulation. In all three colonies, the parties that supported military spending in King George's and the French and Indian wars supported the patriot cause, while those opposed to preparedness became Tories or reluctant patriots. "In the Revolutionary crisis," Egnal claims, "the expansionists, following the logic of

their beliefs, supported independence, just as the non-expansionists, equally consistent, opposed it" (p. 59). Albert G. Olson in his "Empire and Faction: A Comment" cautiously agrees and says that "if the expansionists and their opponents turn out to coincide reasonably well with patriots and loyalists, then Egnal's argument may well open the way to a new understanding of the Revolutionary period" (p. 69). Alas, Egnal and Olson will discover that "expansionist" Thomas Cushing was a vehement opponent of independence, and his ally John Hancock was at best silent until it became inevitable.

David R. Chesnut's " 'Greedy Party Work': The South Carolina Election of 1768" argues that, in spite of clear-cut issues (the Regulators wanted reform, the Charleston mechanics a strong stand against the Townshend Acts), the election of 1768 led to no permanent party structure, and South Carolina quickly returned to domination by the gentry. Joseph A. Ernst's "Another View of the South Carolina Election of 1768 and the Regulators" praises Chesnut's account of the election and subsequent assembly session, but finds the Regulators less ephemeral.

George Dargo, in "Parties and the Transformation of the Constitutional Idea in Revolutionary Pennsylvania," argues that the transfer from unwritten to written constitution undermined the legitimization of parties as protectors of liberty; thus the toleration of parties disappeared during the war. Stephen E. Patterson's comment, "Constitutional Formalism or the Politics of Virtue?" challenges Dargo's "old consensus view of the Revolution" and suggests that "while parties paid lip service to the idea that constitutions should eliminate partisanship, perhaps they really hoped that constitutions would eliminate the other party" (p. 118).

These papers are well presented and probably useful but not, however, very exciting or likely to spark a fierce debate on parties in Revolutionary America.

NEIL R. STOUT
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MARY C. GILLET. *The Army Medical Department, 1775-1818*. (United States Army Historical Series.) Washington: Government Printing Office. 1981. Pp. xiii, 299. \$11.00.

This book will be a valuable addition to the tools of historians concerned with military events in America from 1775 to 1818, and of some historians of medicine. Mary C. Gillett has limited her focus strictly to the operations of the Army Medical Department and its forerunners, weaving together from a wide variety of sources the stories of its few successes and many disappointments.

The first chapter outlines concepts of medicine and therapy prevalent in 1775. The weakest chapter of the book, paradoxically it is neither so understanding nor accurate as the corresponding material in the last chapter. The next four chapters clearly and concisely describe the evolution of the Continental army's medical service and its roles throughout the Revolution. The sixth chapter describes military medical services in the infrequent campaigns of the next three peacetime decades. Three more chapters, devoted to the War of 1812, repeat the story of fiscal and logistic ineptitude told for the Revolution. Clearly, as noted in the foreword (p. ix), in both wars the army's medical department was only "a wartime expedient." Throughout, chapters are organized in respect of geographic campaigns. The last chapter leads up to the establishment of a permanent medical department in 1818. The appendixes include congressional legislation affecting the department from 1775 to 1818, a list of some influential doctors of the Continental army, and a supplementary list of Continental hospitals.

Among the book's major contributions to scholarship are its forty pages of notes and twenty pages of bibliography; they demonstrate great perseverance in ferreting out sources not previously available or seldom exploited since the pioneering work of Louis C. Duncan (1931) or Philip Cash's more recent (1973) study. Twelve useful new maps show the locations of army hospitals in the campaigns covered. Although many illustrations are appropriate, some are only peripherally relevant, and many are of poor quality, especially the portraits.

In general, Gillett has accomplished her goals. Her data amply justify her conclusion that neither the army nor Congress could provide sufficient long-range planning to meet the troops' medical needs. Fortunately for the troops, quarrels among their leading physicians did diminish in severity and consequences from about the middle of the Revolution onward. The book would have been much more valuable, however, had the author's goals been more comprehensive. It is a disservice to have explicitly avoided considering the regimental surgeons; recent research suggests that some of them at least were indeed superior to the opinions of them expressed by their contemporaries in the regular service of the Continental army, and the regimental surgeons did contribute to the care of the troops. Secondly, although Gillett has woven the numbers compiled by Howard H. Peckham, Charles H. Lesser, and Morris H. Saffron into her stories, such sporadic citations are necessarily incomplete and fail to provide information over the entire life span of, for instance, any given hospital. Additional appendixes tabulating or graphing important relevant data, such as weekly or monthly censuses (and diagnoses) for individual hospitals, would have add-

ed much to the book's value and permitted more discussion in the text. This one area highlights the unfulfilled potential uses of statistics in modern historical research.

In summary, this necessary new history of the early American Army Medical Department will be of considerable, but not maximum, value to historians of independent America's first half century of military conflict. The book's copious documentation will be one of its major contributions; its purposeful neglect of regimental surgeons and omission of statistical tabulations provide challenges to future students of the period and its problems.

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PAUL F. DEARDEN. *The Rhode Island Campaign of 1778: Inauspicious Dawn of Alliance*. Providence: Rhode Island Bicentennial Foundation, for the Rhode Island Publications Society, Providence. 1980. Pp. xv, 169.

For the new American nation the year 1778 was more important for what did not occur militarily than for what did occur. In late June General Washington narrowly missed inflicting a stunning blow on Sir Henry Clinton's army at Monmouth. In July Count d'Estaing, commanding a large French fleet, might, with a little luck, have dealt a mortal blow to the British fleet off Manhattan Island. In August a Franco-American force planned a joint operation designed to liberate Rhode Island from British control and, in the process, destroy the seven-thousand-man redcoat army of occupation under Sir Robert Pigot. Coming on the heels of General John Burgoyne's disaster at Saratoga, the success of even one of these endeavors could have led to an immediate termination of the war. Each mission failed, of course, and the war dragged on for another five years.

Paul Dearden scrutinizes the third of these actions, the Rhode Island campaign, in this little monograph. Readers will discover that Dearden offers a well-researched and balanced account of this abortive venture.

Because the allied invasion failed, many previous historians have sought scapegoats. General John Sullivan, the American commander and a field officer guilty of egregious errors in earlier campaigns, most often has been identified as the culprit. But d'Estaing, who ultimately declined to cooperate with Sullivan during the siege of Newport, also has drawn fire. So has the militia force, about one-half of which deserted after only two weeks' service. Washington, too, has been criticized for his refusal, ostensibly for political reasons, to place either Gen-

eral Horatio Gates or General Nathanael Greene in command of the operation. There are no whipping boys, however, in Dearden's judicious narrative.

Dearden seems to feel that the campaign turned on two decisive points. Late in July d'Estaing succeeded in isolating Pigot at the southern end of the island, in and about Newport, but Sullivan, with an undermanned and ill-provisioned force, could not contemplate an immediate attack. Sullivan, Dearden argues, was not to blame for his inadequate force; because of the uncertainty of d'Estaing's prior activities off Manhattan, Sullivan had only a week's notice that an attack on Newport was being planned. The allied delay permitted Pigot to strengthen his fortifications and secure reinforcements. By the time Sullivan and d'Estaing were ready to act in concert, Admiral Richard Howe had arrived, in command of a fleet that was superior to the French both in numbers and in firepower. D'Estaing, fearful that he might be blockaded, sailed away, informing Sullivan that he could return only after he had defeated Howe on the open sea.

Sullivan, nevertheless, continued the siege, and a week after d'Estaing's departure Pigot was hinting at the necessity of surrender. Had d'Estaing defeated Howe and returned, Dearden implies, Newport would have fallen. D'Estaing, however, did not return. The French admiral, his fleet badly damaged by an Atlantic storm as he sought out Howe, decreed that he must sail to Boston for repairs, a decision that Dearden believes was justified.

Dearden's study conveniently outlines the history of the war in Rhode Island preceding 1778. The author also provides a useful chapter dealing with the efforts of Washington and others to placate d'Estaing, whose feathers were ruffled by Sullivan's acerbic comments following the campaign.

While more suitable maps might have been selected, the study is attractively illustrated. The study also might have been strengthened had Dearden devoted more emphasis to the activities of the common soldier. Yet, it is a well-written book, one that specialists in the American Revolution should find useful and informative.

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PETER SHAW. *American Patriots and the Rituals of Revolution*. Cambridge: Harvard University Press. 1981. Pp. 279. \$17.50.

In his preface, Peter Shaw denies that his study of "the Symbolism of American Revolutionary protest" is a "historical interpretation of the Revolution in the strict sense." It offers instead "a reading of its ritual language" that requires an "eclectic approach" drawing upon "the insights of psychoanalysis,

anthropology, and literary criticism" (p. 2). Yet this is a historical work in that it concerns a period of the American past and concludes with a general description of the Revolution's development founded upon Shaw's reading of the period's rituals.

The book contains three major parts, each distinct in character and content. The first, on "Crowds and Crisis," describes the crowds of colonial and Revolutionary America, the colonists' tendency to blame the Earl of Bute and Thomas Hutchinson of Massachusetts for grievances in which they had played no part, and the crowds' heroicization of John Wilkes and William Pitt. Next are essays on four "Conscience Patriots," all from Massachusetts—James Otis, John Adams, Joseph Hawley, and Josiah Quincy, Jr. Shaw is concerned above all with their ambivalence toward authority, particularly paternal authority, whether in the persons of their biological fathers or of persons who served as surrogates, among whom the king is an ultimate victim. He makes no effort to present his subjects as whole persons; and his argument turns too often, above all in the Hawley essay, on circumstantial evidence, questionable inferences, and the transformation of psychological theories into axioms of historical analysis.

By far the most interesting material is contained in Shaw's final unit on "The Revolutionary Impulse," where he notes the importance of young people both as participants and as an identifying symbol in revolutionary demonstrations, then describes the rituals of American crowds, noting their similarities and differences with English practices, and explains the history and cultural functions of popular rituals. The crowds of Revolutionary America, he argues, combined traditional rituals of sacrifice with the rites of passage that had long marked changes of season or young men's coming of age, and so had implicit revolutionary implications. They therefore indicated both submission and resistance, and so expressed well the "psychological crises" of patriot leaders (p. 224). They "initiated a powerful interaction between individuals and the crowd, . . . as if the revolutionary impulse had a kind of anthropological insight, and as if it gained strength from having made the correct ritual linkages" (pp. 225–26). In celebrations such as New England's Pope's Day, Shaw observes a "special emphasis on autonomy and independence" established well before 1760. Above all, in replacing the Guy Fawkes of English ritual with the pope, "Americans had put the father—pope or 'papa'—at the center," and such "scapegoats referred ultimately to the king, as 1776 eventually made clear" (p. 231).

Maybe. But the individual leaders whose psychological crises Shaw described had no profound communion with the crowd: men like Adams and Quincy were deeply suspicious of popular uprisings.

And the pope was no father figure in Protestant America. He was considered an enemy of the British king no less than Guy Fawkes had been, such that antipapal rituals required profound alteration before they could become antimonarchical. In any case, Puritan New England was not America.

Shaw's interpretation of the Revolution is curiously like that of the Progressive historians, who also traced the Revolution to unspoken forces. Like them, he rejects the "legal and political arguments advanced in patriot writings" as "flawed in logic," and sees no real grievances behind colonial discontent (p. 1). British revenue measures were "well thought out, reasonable directives" (p. 230). Events that so preoccupied Americans between 1765 and 1776 recede to insignificance before the power of ritual: "in the attack on Hutchinson it was not the relatively minor issues of the Stamp Act and other revenue bills that mattered, but the coming to birth of a new nation" (p. 216).

But events are essential to explain why independence occurred when it did—to explain when rituals that both expressed and contained a society's anti-authoritarian impulses, and so served the cause of stability, became overtly revolutionary. A study of revolutionary ritual should therefore consider the complex interplay of unconscious with conscious components of political change and should study the "extra-logical" against that logic of revolution that historians have traced with such meticulousness in the post-Progressive era. It should also address itself to the transformation of ritual over time as well as by place, for the rituals of Virginia and South Carolina were distinct from those of Massachusetts. The topic demands, in short, a more self-conscious and rigorous work of historical analysis than Shaw provides.

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STANLEY J. IDZERDA *et al.*, editors. *Lafayette in the Age of the American Revolution: Selected Letters and Papers, 1776–1790*. Volume 1, *December 7, 1776–March 30, 1778*; volume 2, *April 10, 1778–March 20, 1780*; volume 3, *April 27, 1780–March 29, 1781*; volume 4, *April 1, 1781–December 23, 1781*. Ithaca: Cornell University Press. 1977–1981. Pp. xliii, 487; xli, 518; xxxix, 577; xlv, 538. \$22.50; \$22.50; \$35.00; \$38.50.

Marie-Joseph-Paul-Yves-Roché-Gilbert du Motier, Marquis de Lafayette, was born in 1757 and died in 1834. He married and became captain of dragoons before his seventeenth birthday; he joined the Freemasons and became a father before his twentieth. Secretly leaving France against the wishes of his king and family, he established himself before the end of

1777 as a valuable general officer on Washington's staff. Courageous, enterprising, and intelligent in warfare, Lafayette especially distinguished himself in Virginia where he contributed to the capture of Cornwallis. He also played a less easily measured but clearly valuable role in fashioning and managing the Franco-American alliance. With peace restored and his fame established throughout Christendom, Lafayette settled in Paris where he labored to strengthen ties between the United States and France and to reform his own nation. Inevitably he played a leading part in the successive revolutions in France until driven into exile in 1792. He then suffered a five-year imprisonment by France's enemies. Finally restored to his French citizenship and properties, he divided his later years between agriculture and public service.

The four volumes reviewed here will be followed shortly by two more. When complete, this edition will fall very short of covering the life of Lafayette but will certainly cover the years 1776–90 in which he helped win the independence of the United States and shape its growth. Edited, published, and financed in the United States, these large and generously illustrated books are thoughtfully designed for American readers. Therefore, all documents originally written in French have been translated into English, with the original texts supplied in slightly smaller print at the end of each volume. This feature, so useful in itself, incidentally creates an odd stylistic discrepancy. The young Marquis learned English very quickly and was writing to Americans in their own language almost from his first days on these shores. But his letters in English, as faithfully reproduced here, inevitably read as if they were rather inept translations from the French, while his French letters, ably translated by these excellent editors, appear in the most fluent, correct, and accurately spelled modern English!

Stanley J. Idzerda and his staff were surely wise to decide on printing a selection comprising about two-thirds of the Lafayette documents available for this period. Lafayette wrote faithfully to many friends and relatives in France, as well as to many leading citizens in the United States. Brimming over with news, inquiries, and new projects, his letters taken *seriatim* contain a stupefying amount of repetition. A further cause of this was the unreliability of the transatlantic mails in time of war; Lafayette would write virtually the same letter several times to send it by different ships. But granting the principle of reducing duplication, one may still cavil at specific choices. The editors omit much of the extensive correspondence that passed between Lafayette and Alexander Hamilton during the war; we know this because they calendar omitted items in appendices. To be sure, both sides of the Lafayette-Hamilton correspondence appear in Harold C. Syrett, *The Papers of Alexander Hamilton* (1961–), but importance

rather than availability seems to be the criterion for selection here. It may be argued that Hamilton lacked political importance at that time, but then the correspondence may still be valuable for the tone that the two ambitious young hotspurs take toward each other.

In general, however, the editors have chosen a generous number of letters to Lafayette for inclusion, along with "third-party" documents, or portions thereof, that bear significantly on Lafayette's career. The editors have also done a remarkable job of restoring many of Lafayette's papers. In his later years the Marquis carefully edited his writings, and it was in their altered form that his son, George Washington Lafayette, published them as *Mémoires, Correspondences et Manuscrits du Général Lafayette* (1837–38). Obviously the documents are more useful to historians in their original form. The editors have also exercised imagination in dividing Lafayette's long *Memoir* of 1779 into six parts, each of which serves as a substantial headnote to subsequent groups of documents.

Idzerda's introductions are brief, factual, and gracefully written. This is one of those editions that presents its documents to the reader largely on their own terms, without a surrounding structure of interpretation and historical controversy. Yet a view of Lafayette does emerge from reading these volumes. The editors are more inclined to take Lafayette on his own terms than was the late Louis Gottschalk; thus an introductory note on the pursuit of fame in the eighteenth century quite properly reminds us that it was the universal occupation of public men and was understood within the context of virtue. It follows that what distinguished Lafayette from so many of his contemporaries was not his passion for fame but his intelligence, his imagination, and above all his energy. One of the associate editors of these papers, Lloyd S. Kramer, has extended this constructive view of Lafayette in a recent article, "America's Lafayette and Lafayette's America: a European and the American Revolution," *William and Mary Quarterly* (1981).

For students of Lafayette, this edition is but a fragment, though a magnificent fragment, of the record of his life. But it does show us how Lafayette won fame and glory in America and follows the radical young nobleman through the most successful phase of his career. Its greatest value will be for students of the War for Independence and the establishment of the United States, for Lafayette was a tireless commentator on everything from military tactics and personalities to diplomacy and constitutional reform. Besides being an expert and informed witness, he has the inestimable merit of being interesting.

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FRANK LAWRENCE OWSLEY, JR. *Struggle for the Gulf Borderlands: The Creek War and the Battle of New Orleans, 1812–1815*. (University of Florida Book.) Gainesville: University Presses of Florida. Pp. vii, 255. \$20.00.

For those who revel in American military history, here indeed is a well-documented, clearly written, authoritative account of the Creek Indian war and the battle of New Orleans, 1812–1815. It is, in essence, the saga of Andrew Jackson's campaigns against the southern Indians and British. The narrative, based upon exhaustive investigation of American, British, and Spanish manuscript and printed sources, is a history of war and intrigue in which Jackson gradually emerges as the central figure.

Frank Owsley's main argument is that conventional history has placed too much emphasis upon the northern, Canadian side of the War of 1812 thus tending to overlook the fierce struggle for the Gulf borderlands. He maintains, with considerable justification, that the Creek war was especially significant because it began as a separate operation and gradually merged with the fighting against the British. The Spaniards, the sources tell the author, encouraged Creek hostility toward the Americans because of a fear that the United States might seize all of Florida; but Spain was unable to arm the Creeks so as to prevent their defeat by Jackson at Horseshoe Bend in the spring of 1814. In the summer of that year, the British landed forces at Apalachicola and made plans to arm and assist the Indians. By this time, however, the Creek people were unable to make a sustained effort against the Americans. Many of them were starving (Jackson had followed a scorched-earth policy in destroying Indian towns, fields, accumulated food supplies) and willing to surrender.

Jackson seems to have been marvelously successful because of his extraordinary talents as a strategist and because he made sure that he was never at a loss for intelligence. He used Indians to fight other Indians and to fight the British. And, of course, the warriors often acted as his scouts in wilderness campaigns. It is not surprising that the astute Jackson also took advantage of British errors in military matters. He even appeared to sense when the British generals were about to exercise bad judgment.

The first hand accounts of battles, especially the Battle of New Orleans, quoted by Owsley, are full of dead soldiers' bodies and bloody incidents, a depressing part of our violent past. Jackson, the man, with considerable evidence, is justly portrayed as anti-Indian and author of the plan to remove the Creeks and take over their lands. With the Indians gone, there was no buffer to protect Spain, and Spanish Florida became an easy plum. Both the

Creek cession and the annexation of Florida were rightly credited to Jackson, and the general did indeed find his national reputation enhanced. Almost at once he became the leader of Western expansionist political forces and a new frontier ethos that seemed to sweep the nation.

Here, then, is an excellent military narrative that details the story of frontier growth. Frederick Jackson Turner would like this book. But Turner, who had little understanding or appreciation of Indians (or other minorities) would probably not approve of another kind of book that could be written on the Gulf borderlands struggle. This book would be written from the Indian side, making exhaustive use of ethnohistorical sources to show the impact of dispossession on native American people. This kind of history, to be sure, is as much American history as the saga of Jackson's military exploits.

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DARWIN H. STAPLETON, editor. *The Engineering Drawings of Benjamin Henry Latrobe*. (Papers of Benjamin Henry Latrobe, Series 2, number 1.) New Haven: Yale University Press, for the Maryland Historical Society. 1980. Pp. xx, 256. \$62.50.

Here is a welcome addition to the literature on the history of engineering in the United States. Benjamin Henry Latrobe is generally and understandably known almost exclusively in terms of his architectural creations. These place him in the front rank of the architectural profession during the early decades of the American republic, and the variety, richness, and wide range of his achievement have been dealt with in appropriate length and with great discernment in Talbot Hamlin's *Benjamin Henry Latrobe* (1955). This was based principally upon the voluminous Latrobe papers, now among the leading collections of the Maryland Historical Society in Baltimore. For some years the society has been engaged in arranging, editing, and publishing the Latrobe materials. Although there are innumerable references in the Hamlin work to Latrobe's engineering and related interests and activities, these have understandably a secondary and quite incidental place. Only at the very end of the biography does Hamlin devote a brief chapter to "Latrobe as Engineer," although two earlier chapters covering Latrobe's Pittsburgh experience in steamboat building fall chiefly within the same category.

The present, beautifully illustrated work may best be described as an introduction and source book, a concordance of sorts, of the documentary materials resulting from Latrobe's secondary career as an engineer. The drawings are the central feature,

embodying as they do the engineer-author's ideas, his proposals for dealing at large and in detail with each engineering project. Fortunately, in the case of Latrobe, the bare bones presented here may be fleshed out with the aid of the extensive verbal records of the Latrobe papers, already available in Thomas Jeffrey's edition of *The Papers of Benjamin Henry Latrobe: The Microfiche Edition* (1976).

The admirable, indeed, indispensable *vade mecum* to the present volume is provided by editor Darwin H. Stapleton's introductory essay, which traces the course and content of the subject's career and will serve admirably as an extended biographical sketch, supplemented by an earlier chronology, centering in the engineering work, until a full-scale biography of the subject as engineer may be forthcoming. The account is arranged under various headings: transportation, principally river improvements, canals, roads, and bridges; urban waterworks; river control; and what here are termed "industrial works." Under the last are included Latrobe's steam engine building activities and the important, if in some respects disastrous, participation in the steamboat promotion and building ventures of Robert Fulton and associates. Under a final section are gathered a miscellany of matters relevant to engineering and a stimulating concluding discussion of "The American Technical Community."

To Latrobe, as to many of his generation, power meant pre-eminently steam power; and the steam engine, which he came to regard as a specialty, was to be promoted and applied in his practice at every opportunity. Steam power played a central role in three of the most important projects of his engineering career. These were the Philadelphia and New Orleans waterworks and the steam navigation of the Ohio River. The last of these three, the period 1813-15 at Pittsburgh associated with Fulton in steamboat building, was one of mounting difficulties and eventual estrangement from Fulton, dismissal from office, financial embarrassment attending the unfinished steamboat, and a general depression of spirits, from which Latrobe was eventually rescued by a government commission to restore the war-damaged capitol buildings at Washington. The correspondence revealed Latrobe as a staunch supporter of Boulton and Watt orthodoxy in steam engineering and a vigorous critic of Oliver Evans and followers, advocates of high-pressure steam engineering.

Latrobe fancied himself, and with good reason, an expert in steam engineering, bringing from England a certain familiarity with Boulton and Watt practice, reinforced in this country by the expertise of certain former Boulton and Watt employees and that of Nicholas Roosevelt, an associate of Fulton in his steamboat ventures who was to become Latrobe's son-in-law. On most counts the Philadelphia waterworks was the most impressive of Latrobe's engi-

neering experiences because of the varied and large structures required, the amount of capital investment, and the social benefits anticipated for the population of one of America's largest cities. The completed waterworks, runs the present account, was not only successful in meeting the city's needs for water, ending reliance largely on contaminated wells and contributing to the decline of epidemic yellow fever, but also had "far-reaching effects on the course of technological development in the United States." It was, moreover, "the most important engineering commission of [Latrobe's] career."

Unfortunately, these rather large assertions are without substantial support. The actual events added little to Latrobe's reputation as an engineer, failed at acceptable cost to resolve the city's water supply problems, and emphasized in a dramatic way the limitations of steam power at this time in dealing with its larger applications in stationary use. The decisive evidence of failure is found in the published reports and associated actions of Philadelphia's "Watering Committee," the municipal body responsible for the development and operation of the city's water supply system. From the outset the system was operationally troublesome and a heavy and unanticipated financial burden. Some relief was obtained by the replacement in 1815 of the entire power plant, pairing a locally built Boulton and Watt engine with one built by Oliver Evans that proved of superior performance. But the new plant, too, after several years was abandoned. The Watering Committee marked paid to the steam power account, imported an expert from New England, threw a dam across the Schuylkill River, purchased the necessary water rights, and with the completion of the first set of massive wooden breastwheels—the type being introduced in Lowell's first textile mills—began operations in 1822. Overnight the trouble, trials, and financial burden of the old system seemed to disappear. Such results surprised few; they simply confirmed the general expectation and supplied support for the long-persisting view that steam power was an expedient of the last resort and suitable only in minor applications.

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THOMAS C. COCHRAN. *Frontiers of Change: Early Industrialism in America*. New York: Oxford University Press. 1981. Pp. 179. \$15.00.

"Amerika, du hast es besser," Goethe said, and for at least 150 years nearly everybody who has been concerned with the rise of American civilization has been trying to explain why we had it better. Laid aside, in their turn, have been such simplisms as free land, vast natural resources, germ-of-European-civilization, and Turner's frontier and such contributions from the "new" economic historians as the wrangle over relative costs of labor and capital. Sooner or later, most historians sweep the lumpy problem under the rug with all the eagerness with which Karl Marx accepted free land as a convenient reason why an American proletariat would probably not develop.

But not Thomas C. Cochran. Drawing on his long and distinguished career as a student of American economic and business institutions, enriched by nearly ten years of "retirement" in which he has stayed busy at such activities as helping to guide the Regional Economic History Center of the Eleutherian Mills–Hagley Foundation, he has produced an excellent little book that places the emphasis squarely upon the unique cultural traits of the American people themselves. Rapidly increasing in numbers, enterprising in the very act of emigration from the old continent, and pragmatic in the connection they made between theory and practice in the arts and crafts, Americans above all were willing continuously to occupy the "frontiers of change," in Cochran's apt term. Being free to create a new society, they accepted change as the natural order of things. (Just when and why this trait weakened as the full implications of the industrial age dawned, is a topic toward which this book points the way for future work.)

Cochran emphasizes that nineteenth-century American society possessed, and European society notably lacked, a large, rapidly growing domestic market intelligently organized and led by men of a common purpose in a polity that was able to become continentally integrated whenever technology and enterprise should make it feasible. Along with this went the clean slate of a social environment in which anyone could become a capitalist. Many thousands did just that, thus making the owner-manager, in whom the nexus of power and accountability was optimal, the dominant feature of American material culture. Foreign trade, the role of which has been exalted by Douglass C. North and others, is far less important in Cochran's view.

Inevitably, as he tours such traditional institutional topics as finance, law, technology, and industrial organization in his plain, sensible prose, Cochran sometimes gives the impression of superficiality, but his notes and bibliography reveal a mastery of the sources that will inspire others to emulate it. This is the best historical study, narrating a continuous web of cause and effect, as opposed to a merely descriptive statistical study, that we have of American economic growth between independence and the Civil War. All scholars of Western civilization can benefit from this fresh vista of an era in which a truly new political economy was created. *Frontiers of Change* deserves to be widely used in survey courses that try to recreate for students the real America,

and it is ideal for courses in social and economic history, both graduate and undergraduate.

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DANIEL WALKER HOWE. *The Political Culture of the American Whigs*. Chicago: University of Chicago Press. 1979. Pp. vii, 404. \$23.00.

The American Whigs—the party of John Quincy Adams, Clay, Webster, Lincoln, and more than a million loyal voters—deserve a book that does not make them shadowy intruders upon the age of Jackson. Daniel Walker Howe has written a respectful, even glowing, account of their distinctive lives and thoughts and works, portraying them at once as faithful guardians of political and moral tradition and as bold apostles of a brave new economic world.

Howe emphatically reminds us that the Whigs, over the brief span of some twenty years, enlisted more than their share of America's political intelligence and eloquence, served a broad and varied constituency from their New England heartland to the Deep South to the New West, defined important policy alternatives for the nation with persuasive logic, largely staffed and guided the agencies of social reform, quickened Old Whig traditions in apt response to new public problems, and bequeathed an enlightened and refurbished Whig tradition of their own to Republican, Mugwump, and Progressive heirs—and perhaps to troubled postmoderns as well.

The short, frustrated life of the Whig party, Howe suggests, tends to obscure the long, honorable, and triumphant career of Whig “political culture” in American history. Thus he addresses his work to that dimly recognized “political culture,” using representative men, works, measures, and campaigns as illustrations of his larger theme. Lyman Beecher in the pulpit or Rufus Choate at the bar have at least as much to say about the central meaning of Howe's Whiggery as do old war-horses like Adams, Clay, Webster, or Seward. The local hacks and oracles who bring party business home to the neighbors figure not at all in Howe's account.

The choice of “political culture” for his title and analytic principle seems natural enough in an age of sociological and psychological historiography. Ideas seem real only in the form of “ideologies.” Minds become substantial as “mentalities” (yet more profound in French translation) or as “mind-sets.” Arguments acquire historical weight when rendered as—if I may be forgiven the expression—the symbols of “persuasions.” The whole realm of politics, words and acts alike, becomes a mirror world reflecting, sometimes illuminating, often distorting or masking inner conflicts or social conflicts.

Viewed in this context, *The Political Culture of the*

American Whigs, after invoking all the fashionable authorities by way of methodological introduction, reads as a rather old-fashioned study of political and social thought, drawing arguments straightforwardly from texts, distinguishing rhetorical modes, tracing intellectual genealogies through the ages, defining manifest political purposes and strategies, and only incidentally noting the cultural significance of Lincoln's constipation or Greeley's re-enactment of Poor Richard's progress.

Why, then, should Howe attribute not merely a political style or mood but a full-blown “political culture” to a party that endured less than a generation? There is little about the minds and characters, origins and careers, of the highly diverse Whig leaders and spokesmen that Howe portrays to suggest convincingly a deeply shared social experience forming common cultural bonds. Nor does the relatively brief life of their party as it took various shapes in Brahmin Boston, upstate New York, downtown Philadelphia, Illinois, or Georgia reveal the pervasive presence of a homogeneous Whig political community that could nurture and sustain a culture of its own.

On the face of it, Howe's Whigs seem to be united precisely as a political party that more or less agreed, or could tolerate their disagreements, on candidates, strategies, and programs. At least, they recognized the common Democratic enemy. The American System of Clay and Henry Carey or the Spot Resolutions of Lincoln or the Log Cabins of old Tip's triumphant campaign against the Fox, it appears, drew Whigs together more compellingly than Choate's religion of the Common Law or Lyman Beecher's New School faith or Horace Greeley's pharmacopeia of social cures. Insofar as Howe suggests a cultural depth for Whiggery by looking backward to classical, British, colonial, and Revolutionary traditions or forward to a burgeoning American Victorian culture, he tends to submerge his concrete historical subject—the Whig party, ca. 1834–54—in a boundless sea of diffuse ideas and symbols.

The chapters range from disappointingly thin sketches of such complex and powerful figures as Adams and Lincoln, to a suggestive upward revaluation of Greeley's gifts as social critic, to a revealing exploration of the shaping role of rhetoric and law for Choate and Webster. The last indeed touches directly on the cultural dimension of politics and gives Howe a fair chance to follow his intellectual bent. The juxtaposition of the diverse figures and themes of Whiggery helps to define the party's range and to pose provocatively the question of the party's common character and outlook, even if it does not reach wholly satisfying answers in the chosen terms of a Whig “political culture” or of an ideal-type of personality.

Among the broad conclusions of the work, I find most to ponder—not surprisingly—in the proposition that Whigs, in contrast to Jacksonians, defended conservative social and political values derived most open-mindedly from Cicero and the Florentines, from Harrington and Burke, the Common Law and the “country” tradition, from Scottish common sense and evangelical-millenarian enthusiasm, perhaps (although the point is strangely slighted) from a mix of Madison, Hamilton, Jefferson, John Adams, and John Marshall—while embracing a radical logic of economic growth and change that overleaped Adam Smith on the fast track to modernity.

MARVIN MEYERS
Brandeis University

ARLIN TURNER. *Nathaniel Hawthorne: A Biography*. New York: Oxford University Press. 1980. Pp. xii, 457. \$20.00.

Ever since Henry James, biographers of Hawthorne have had a way of becoming rather tendentious and theoretical about their mysterious subject. Handicapped by the lack of many necessary materials, like his letters to Melville, the biographers have built sometimes ponderous theories about the significance of questions that have perplexed them to begin with—his shyness, his ambiguities, his chilly relations with Emerson, his sense of his Puritan ancestors, and even his sense of America.

This biography, instead, attempts to be just a calm and detailed account of Hawthorne's life. It barely theorizes at all; it chronicles. Since it does so with many documents not used before and handles the information fully and dispassionately, it should be interesting both to people needing the facts and to people now turned off by more argumentative interpretations. It also devotes attention to several periods of Hawthorne's life that have not been so well known.

One of these periods is the early years of his marriage to Sophia Peabody, preceded by their five and a half years of courtship and marriage, 1837–1842. Because of his legendary difficulties in earning a living from writing, Hawthorne had to wait to marry until he was thirty-eight. But then happiness came, Sophia writing in their joint notebook that the human body is a “miraculous form” and that “the truly married alone can know what a wonderful instrument it is for the purposes of the heart” (p. 153). This was not a frustrated and unfulfilled Victorian marriage! But it is equally apparent that Hawthorne strove tirelessly to keep his formerly sickly wife comfortable and to support his family respectably. During the last three years before the marriage, he had had barely any time to write, and

this was but the first of a number of long forced dry spells.

A second period that Turner digs out in detail is the end of Hawthorne's stay in the Salem custom-house, when local Whigs broke their promises not to dismiss him. One particular villain of that experience was Charles W. Upham, “a liar and a most consummate hypocrite,” according to Sophia (p. 180), and so this prominent minister of the First Church became the major model for Judge Pyncheon in *The House of Seven Gables*.

A third well-examined period is Hawthorne's four years as consul in Liverpool. That was then “the most lucrative post in the foreign service,” Turner says (p. 257), and Hawthorne saved money for his later travels, though this also meant severely restricting his travels in England. And yet thrifty as he was, Hawthorne was still generous with both time and money in helping stranded sailors and in urging improvements in the laws protecting their rights.

Thus Hawthorne emerges as shy and hard-working but also politically astute and politically liberal (except in not doing much to oppose slavery). But unfortunately, more information is available on Hawthorne's life as husband, politician, and consul than his life as author.

ROBERT F. SAYRE
University of Iowa

RAYMOND J. O'BRIEN. *American Sublime: Landscape and Scenery of the Lower Hudson Valley*. New York: Columbia University Press. 1981. Pp. xii, 353. \$19.95.

American Sublime is a historical account of the development of the activities of and attitudes toward the lower Hudson Valley during the nineteenth century. Somewhat unsuccessfully attached to this study is an introductory chapter that attempts to prove that the Hudson is “America's River” and that Storm King and other environmental struggles are prototypical and significant in the unfolding of American environmental history.

The substantive body of the book is an interesting case history of nineteenth-century development of commercial interests and aesthetic sensibilities about the Hudson. Raymond J. O'Brien has used artwork, sketches, photographs, and paintings as well as local histories, maps, guidebooks, and numerous secondary sources. He seems to possess such an embarrassing wealth of materials and has used so many of them that the organization becomes loose and overlapping in places. Also the reader often becomes confused by place names. A detailed reference map, especially drawn to include the major geographic locations of the book, would have been a helpful addition.

Beyond the excellent descriptive local history of attitudes and activities, the argument of the text contains a few methodological problems. It is clear, for example, that the author wanted to show changing and often conflicting attitudes toward the landscape. But the author does not clearly distinguish, or incorporate into his argument, the differences between the elite classes whose minority opinions are usually registered by history and the people who actually lived in the region. Travelers, foreigners, artists (and art historians), politicians, and even developers all had their own special reasons to extol the beauties of nature, beyond the intrinsic value of nature itself.

O'Brien's text is interesting throughout because of his effort to show how mining, industrial, and real estate development slowly changed the face of the Hudson River Valley. He did not, however, link this activity with progressively conflicting attitudes, especially between natives and outsiders—including state and federal governments—over how the land should be used.

What actually occurred in the Hudson Valley, as elsewhere in the United States, was that the inhabitants of the region manifested attitudes in favor of the economic development of the land. Their livelihoods from farming, mining, transportation, and real estate development depended on economic development, and thus it is not surprising that they should have supported such "unaesthetic" uses of the land. Mainly outside interests, from government and philanthropic sources, worked for preservation. A more suggestive story of the historical geography of the Hudson would have specified what attitudes, interests, and ideological traditions came into play and for what reasons during the last century.

Instead, O'Brien depended a great deal on intellectual and geographical historians such as Merle Curti, Ralph Brown, Donald Meinig, Roderick Nash, and others, even though he was in a better position to establish his own independent thesis (since his command of the materials is strong) and judge other, more general hypotheses by his own criteria and data. The result of his research, therefore, seemed to turn into a descriptive account interspersed with the conclusions of dozens of other historians. Furthermore, the reader does not get clearly delineated purposes and goals in the introductory chapter, perhaps because the publisher wanted the book pitched to the popular market.

The author's dependence on outside opinions occasionally leads him to faulty conclusions of his own. For example, based on the assumption of the "Judeo-Christian tradition of repugnance for landscape . . . that was put ashore with the Mayflower" (p. 77), O'Brien postulates this same prevailing attitude in the valley during its early history. The idea may have been typical of the Puritans, who

attributed diabolic presence to wilderness areas, but it is hardly the commanding Christian tradition. The biblical books of *Wisdom* and *Psalms* set a stronger tradition for the harmony and beauty of nature recognized in medieval and later thought.

The book would be enjoyable reading were it not for the troublesome problems of historical methodology. When a study treats ideas as well as economic and social history, it is important for historians to indicate what kinds of materials they use, how ideas and attitudes are linked with ideological values and traditions or economic activities, what the role was of class interest, and whether technological progress played a role. Such methodological specificity and clarity would have helped those who do similar studies in the future.

JOSEPH M. PETULLA
University of San Francisco

FRANK N. SCHUBERT. *Vanguard of Expansion: Army Engineers in the Trans-Mississippi West, 1819–1879*. Washington: Government Printing Office. 1981. Pp. xii, 160. \$4.50.

Frank N. Schubert's *Vanguard of Expansion* is a compact yet comprehensive narrative of U.S. Army exploration in the West in the nineteenth century. More specifically, it deals with the activities and exploits of the engineer explorers (or "topogs" as the author calls them) of the Corps of Topographical Engineers from Major Stephen Long and the 1819–20 "Yellowstone Expedition," which may have been at least partially responsible for the creation of the corps, to Lieutenant George Wheeler and the expeditions of the civilian scientists of the 1870s, which marked the end of the corps' relevance for Western exploration. Schubert discusses, chronologically for the most part, the early period of Long, Schoolcraft, and Nicollet; the "pathfinding" of John C. Fremont; the reconnaissances of the Mexican War; the boundary surveys along the Canadian and Mexican borders; the search for a railroad route to the Pacific; the surveys of the Indian wars; and the "great surveys" of the 1870s.

The author, a historian with the U.S. Army Corps of Engineers, has utilized source material that is, in the main, in published form, and he has made no startling new disclosures of previously undiscovered documents or maps. His handling of the major characters of the book and the times in which they acted is conventional with no fresh historical insights or interpretations. The work deals more with explorers than with the systematic process of exploration, although the author does attempt to discuss briefly the significance of the topographical engineers for the expansion of geographical knowledge and occasionally refers to the practical results of the

corps' exploratory endeavors in terms of roads, railroad passes, telegraph routes, and fort locations. In spite of a rather prosaic treatment of a vital and exciting process, Schubert does make several points that are significant to an understanding of Western exploration in the nineteenth century: the failure of the corps to be much in the public eye was largely the result of the government's inability to articulate an official policy regarding Western exploration; the adulation received by individual members of the corps (if not the corps as a body) was the consequence of their outstanding character (many of the "topogs" had been at or near the top of their West Point class); and, despite the apparently episodic nature of the corps' explorations, the experience of the topographical engineers formed a coherent link between the informal explorations of the fur trade and the civilian scientific expeditions of the post-Civil War era. To the corps fell the task of filling in the blanks on the map of the West, and they performed their task admirably.

Vanguard of Expansion is far from a definitive work on the subject, and readers interested in comprehensive analysis would do better to consult the works of William H. Goetzmann. Yet, for those readers with a more casual interest, Schubert's slim volume will prove attractive. It is an authoritative, well-written, handsomely illustrated, and concise treatment of a fascinating and important group of Western explorers.

JOHN L. ALLEN
University of Connecticut

BENSON LEE GRAYSON. *The Unknown President: The Administration of President Millard Fillmore*. Washington: University Press of America. 1981. Pp. iii, 175. Cloth \$17.00, paper \$8.75.

This brief study of Fillmore's life and presidency may be useful for anyone wishing a short factual reference book on Fillmore, but it is marred by several inaccuracies. Nicholas Trist, not Charles Trist, negotiated the treaty of Guadalupe Hidalgo. Henry Foote did not charge into the Senate chamber brandishing a revolver. He drew it in self-defense when accosted by the much older but larger Thomas Hart Benton, who was justifiably incensed by Foote's personal insults that Fillmore as presiding officer should have declared out of order. The anti-slavery Know-Nothings did not nominate Fremont for president in 1856. They nominated Nathaniel Banks, who, according to a prearranged plan, withdrew after the Republicans had nominated Fremont. The political skills and money applied by Fremont's friends to prevent his nomination by the Know-Nothings without losing their later votes deserve to be remembered.

Grayson argues that Fillmore's lack of a solid reputation stems in large part from his efforts to secure passage of the Compromise of 1850. Fillmore contributed only minimally, however, to the event that did most to pass the compromise—the parliamentary mix-up that split the bill into its component parts. Numerous leaders had predicted from the beginning that the measures would pass easily if voted on singly; each could command a separate majority, but very few lawmakers could support all of them. The Southern effort to keep them in one bill was an attempt to gain most of present-day New Mexico for Texas in exchange for the admission of California. Grayson does not cite the *Congressional Globe* in his discussion of the 1850 debates and apparently did not study this original record. Also, in recent years the leaders for the compromise have been praised highly for their efforts, and many historians have considered Fillmore's role his most important achievement.

The other blot on Fillmore's record, his acceptance of the Know-Nothing nomination for president in 1856, is treated by Grayson with objectivity and critical understanding. Some have argued that Fillmore and some of his supporters were merely trying to reconcile the North and South by giving them the foreign Catholic for a common enemy, but Grayson makes it clear that Fillmore shared many of the movement's prejudices. As Grayson insists, Fillmore probably did see the Know-Nothing movement as a possible instrument for sectional peace, but the party's principles caused him no serious inner conflict.

Grayson, a retired foreign service officer, admires Fillmore's foreign policies, and Fillmore's restraint in various minor crises with Latin America, Britain, Spain, and Austria was indeed commendable. His prestigious secretary of state, Daniel Webster, often disagreed with the president and was very difficult to control. A president with a stronger political base might have dismissed Webster, but Fillmore was probably wise in his practice of undermining the secretary without direct public confrontations. Fillmore also began the process of opening Japan that culminated in Perry's success during the following administration.

Grayson ultimately concludes that, while Fillmore was no giant, he deserves better historical treatment than he has received. This may be true, but it is debatable. Fillmore has rarely been harshly criticized, and if he has received only minimal attention and praise, it is because he could not use the power of his office to emerge as a strong leader for either the North or the South or for the cause of sectional peace.

ELBERT B. SMITH
*University of Maryland,
College Park*

PAUL FINKELMAN. *An Imperfect Union: Slavery, Federalism, and Comity*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1981. Pp. xii, 378. Cloth \$22.00, paper \$12.00.

The subject of this forthright, solidly researched book is the legal conflict that arose over the issue of slave transit from 1787 to 1860. At the beginning of the republic, Northern states allowed slave owners to bring their slaves with them while sojourning within a free state, usually for a period up to six months. By the same token Southern states recognized the force of free state law by awarding freedom to slaves who had lived or visited at the North and then returned to the South. Legally the situation was one in which free and slave states, under the doctrine of comity, recognized the existence of rights derived from the laws of the others. In the 1840s, however, intensification of sectional conflict led to mutual rejection of comity in the matter of slave transit. Henceforth Northern states, except for New Jersey, Indiana, and Illinois, emancipated sojourning slaves, while Southern states ruled that slave status reattached to slaves who had resided or visited in free jurisdictions. Numerous cases illustrated the rejection of comity in the 1850s, showing that long before secession occurred the Union was divided into two irreconcilably opposed groups of states.

Although in its essential elements this story has been familiar to students of constitutional history and the antebellum period, Paul Finkelman's account contains much new evidence that will undoubtedly cause it to be considered the definitive treatment of this narrow phase of the constitutional controversy leading to the Civil War. Whether the book illuminates the larger question of Civil War causation, however, as its author apparently intended, is more debatable.

Finkelman argues that Northern leaders like Lincoln were not making things up out of whole cloth when they talked about a future Supreme Court decision legalizing slavery in the free states, just as the *Dred Scott* decision had legalized it in the territories. For in 1860 a state case was available to serve this very purpose. In *Lemmon v. The People* (1857, 1860), the New York courts permitted the emancipation of eight slaves who had been brought to New York City in transit from Virginia to Louisiana. This case never came before the U.S. Supreme Court, but Finkelman speculates that if it had, the Court might well have reversed the New York decision and upheld a federal right of slave transit. It thus would have established slavery in the North. Finkelman acknowledges that no conspiracy existed to promote this end, and he concedes that no one seriously thought a "full-blown" slave system could be forced on the North. Nevertheless, observing

that precedent-setting cases are not always planned, he concludes that the possibility of nationalizing slavery through a series of Supreme Court decisions existed. And even if it did not, he adds, many Northerners *believed* that it did.

It is hard to disagree with this line of reasoning, but like most hypothetical—or, as Finkelman prefers to call it, counterfactual—arguments, it has an inconclusive quality about it. Finkelman's main point is that Northern fears of slavery expansion into the free states were not absurd or unfounded. Aside from Northern assertions that their fears were realistic, this judgment rests on the indisputable but not necessarily illuminating fact of contingency in history, plus knowledge of the Supreme Court's proslavery course in the 1850s. On the eve of the Civil War, Lincoln often referred to the legal tendency toward the expansion of slavery that was apparent in recent public policy, and Finkelman's book is essentially an elaboration of this theme. Despite the new information it presents, however, its argument is at bottom conjectural. The possibility that the Supreme Court, in *Lemmon* or some other case, would have legalized slavery at the North becomes in Finkelman's mind a virtual certainty. Knowing this, he reasons, we can better understand why the Republicans acted as they did. Yet the fact is that the Supreme Court *did not* legalize slavery in this way, nor was it in process of doing so when the war began. The question of whether Northern fears were groundless is thus ultimately a fruitless one because it cannot be answered with certainty.

HERMAN BELZ
University of Maryland,
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ANDREW DELBANCO. *William Ellery Channing: An Essay on the Liberal Spirit in America*. Cambridge: Harvard University Press. 1981. Pp. xviii, 203. \$15.00.

William Ellery Channing, founder of American Unitarianism, was a man of moderate sentiments. How does such a person fare in a society in which extremes become commonplace? Is he condemned either to join one of the extremes or to beat a retreat from his world? Andrew Delbanco knows that these questions are as pertinent to contemporary America as they were to Channing's early nineteenth-century setting, and in this engaging intellectual biography commentary on Channing and his times becomes, by implication, an essay on the fate of liberalism in modern America.

In his rationalist supernaturalism, Channing surrendered neither to a mechanical world emptied of divine presence nor to a nature constantly aswirl with the gods; he committed himself instead to a balance between reason and revelation. In his inter-

pretation of history, he sought to avoid both a mindless endorsement of change as such and any mystical retreat into antihistory; his alternative was to enclose time within the consciousness of the individual. Channing's approach to religious controversy was also a temperate one. For all of his criticisms of Calvinists and other Trinitarians, he insisted that religious differences spring from modes of expression rather than from modes of conviction and that beneath diverse interpretations of scripture lies a basic human harmony.

Delbanco is not the first to construe Channing as a moderate in an immoderate age, but his rendering has more nuances than those of his predecessors. He sees, for example, how Channing's "balanced" interpretation of history was more sensitive to the dilemmas of Boston's intellectuals than to those of the workers in Boston's factories. He also sees how Unitarian pleas for religious tolerance could mask a spirit of self-congratulation and how an antipathy to all extremes could weaken moral resolve in the face of social evil. In fact, Channing himself grew increasingly critical of some aspects of his liberalism. Above all, the institution of slavery called into question unqualified assumptions about human harmony and the balances of reasonableness.

Yet Channing found a way to face radical evil without abandoning the spirit of moderation. Recognizing his own tendency to avoid the brutal facts of evil, late in his life he developed a theodicy based on the conviction that "there is a higher good than enjoyment; and this requires suffering in order to be gained." And despite his distrust of the extreme abolitionists, he eventually engaged in a pointed critique of slavery, fully persuaded that that social cancer grew both from external institutions, North and South, and from evil lurking within every human breast. Channing thus proved that recognition of evil within oneself can simultaneously strengthen the liberal spirit and lead to moral action. As Channing's "self-satisfaction waned, his political engagement grew. He is a key figure of his time because he grasped the moral perils of political commitment and only then committed himself to politics" (p. 142).

There are some unconvincing features of this book. At points it overestimates the role of paradox, ambiguity, and complexity in the work of a religious thinker who was frequently downright simple. It gives little attention to Channing's doctrine of perfection, a doctrine that Channing himself considered a major theme of his philosophy and that clearly pervaded much of his work. And the book draws too tight a correlation between the scriptural hermeneutics of Jonathan Edwards and that of Channing. Still, Delbanco's volume is an important achievement. Against the background of a broadly sketched intellectual and social history, it presents a

subtle and finely detailed portrait of Channing's moderate religious rationalism and thus illuminates the career of an American liberal spirit.

C. CONRAD CHERRY
Scholars Press

LEWIS PERRY. *Childhood, Marriage, and Reform: Henry Clarke Wright, 1797-1870*. Chicago: University of Chicago Press. 1980. Pp. xiv, 359. \$20.00.

This is an unconventional biography of Henry Clarke Wright who was, among other things, an unconventional defender of some Victorian conventions and a challenger of others. It also is a fine book and one of the few recent works of history as intriguing for its literary strategy as for its content.

That is not to say it is easy reading. Although usually clear by historians' standards, Lewis Perry's prose can be compressed and allusive. That style, and the book's emphasis on Wright's loneliness, make for an occasional note of Romantic angst—an appropriate tone for a protagonist who wrote voluminously, often privately, about himself.

But it is the structure, rather than the prose, that is so fascinating about *Childhood, Marriage, and Reform*. It takes as its subject a man whose life, on the surface, seems to have been as linear as any life ever is. Wright moved through a succession of causes, careers, and emotional attachments, some of them failures. He was an apprentice, scholar, clergyman, husband (one of the failures), abolitionist, pacifist, marriage reformer, free trader, spiritualist, and so forth. Yet Perry's narrative is anything but linear. Wright's long European tour in the mid-1840s, for instance, appears both early and late in the book, in spite of the fact that Wright had a quarter of a century left to live after his return to America. Perry does not even begin with Wright's birth, a common starting point for biographers, but rather with his conversion to antislavery in 1835 and with the years of young adulthood preceding it. Perry does end with Wright's death, but the crucial sentences in the next-to-last paragraph, and some of the most moving words in the book, recall Wright's mother, who died when he was a small child. That defiance of chronology is in tension with Perry's interpretation, which begins at the beginning by focusing on Wright's feelings toward his parents, as well as with Wright's own world view, which placed such a premium on childhood origins that one of his manuscripts included an account of life in the womb.

Yet the tension is a creative one since it reinforces a sense of the crosscurrents within Wright and his times. Had Perry simply written about Wright's development as a professional reformer, a spokesman for "uprooted, forward-looking Protestants"

(p. 231), the story could have unfolded in chronological order. And we would have yet another account, largely in the language of modernization, of a person and a society in transition from old to new. That is a story Perry does not ignore. But he also deals with a complicated series of negations and reversals by which Wright converted an impulse or frustration into its opposite. Thus he moved from the ministry to being anticlerical; from aggression to antislavery and pacifism; from sexual frustrations, a transient existence, and a failed marriage to a glorifier of women, domesticity, and sexual control; from concern for death to its denial through spiritualism. The organization of *Childhood, Marriage, and Reform* allows Perry to move back and forth from what was changing within these patterns of Wright's life to what was relatively static. On the one side stood the fluid society of nineteenth-century America and Wright's fluctuating occupations, opinions, and loves. On the other side stood the "permanent, troublesome strata of Wright's mind," psychological sediment deposited in childhood.

Not everyone would agree with Perry's assessment of Northern society and culture. Nor would every reader like his brand of psychology—some because it is not technical enough, others because it is not developmental enough. Still others may feel that Perry was extremely lucky, and atypical as a biographer, in having access to detailed, although incomplete, material on Wright's fantasies and feelings. Criticisms aside, Perry is far more skillful than most biographers in getting at the interplay between constants and change, between psyche, career, and society. Few scholars, moreover, have been as good at treating the emotional meaning of reform without either inflating or demeaning the particular commitment. The world did not need a day-by-day account of Henry Clarke Wright's life, and Perry did not write it. Instead, he was not restrained by the form he chose or the semi-obscure of his subject. He wrote a thoughtful, imaginative book of considerable interest for what it says about the meaning of reform and reformers, about nineteenth-century America, and about the possibilities of biography.

RONALD G. WALTERS
Johns Hopkins University

JAMES LEE MCDONOUGH. *Stones River—Bloody Winter in Tennessee*. Knoxville: University of Tennessee Press. 1980. Pp. xiv, 271. \$14.50.

Stones River (Murfreesboro, in Confederate parlance) ranks as a major battle in the American Civil War. It rivals Shiloh in casualties (24,000 Northern and Southern), in unskilled commanders, wasted opportunities, and indecision. Civil War students have largely ignored this sprawling encounter in Tennessee, fought from December 31, 1862

through January 3, 1863. It was costly but apparently indecisive, essentially a tactical draw. Nothing much changed after Stones River, save that Union General William S. Rosecrans's reputation rose slightly and Confederate General Braxton Bragg's continued sliding. So most scholarly attention went to Vicksburg, Chattanooga, and Missionary Ridge.

Inattention is undeserved. Confederate retreat, coming at the end of Bragg's campaign to liberate Kentucky, depressed the South and boosted Northern morale in the wake of Fredericksburg and Chickasaw Bluffs. And the action itself is worth a close look. Bragg and Rosecrans both blundered—Bragg in not pressing his initial smashing success and later in ordering a suicidal divisional charge; Rosecrans in failing to devise a battle plan as well as failing to counterattack on two enticing occasions. Johnny Rebs and Billy Yanks fought with high bravery to overcome bad generalship.

James Lee McDonough writes sound battle narrative and embellishes it with quotations from personal accounts to provide a "you are there" feeling. Generally effective, these insertions do sometimes divert the reader too far from the main story. And even so devoted a student as McDonough is hard pressed to fill 271 pages. He offers sixty-three pages of prebattle background and adds twenty-four pages of organizational charts at the end.

He has, however, produced a competent, modern account of Stones River, one that fills an important gap and will be widely used.

FRANK E. VANDIVER
Texas A&M University

LARRY E. NELSON. *Bullets, Ballots, and Rhetoric: Confederate Policy for the United States Presidential Contest of 1864*. University: University of Alabama Press. 1980. Pp. xii, 235. \$15.50.

This book is a thorough, clear-sighted examination of an important topic: the Confederacy's policy toward the federal election of 1864. The Northern election confronted Jefferson Davis's government with both an external challenge—assessing and influencing Northern opinion—and an internal challenge—coping with the expectations of Southerners. Given the war weariness and political uncertainty in the North in 1864, the Confederate administration was addressing an area of enormous potential significance. Therefore, Larry E. Nelson's monograph constitutes an important addition to the analyses of Jefferson Davis's conduct of Southern affairs. But its evidence also suggests that the greater despair and war weariness that prevailed in the South were major impediments to the execution of Confederate policy for 1864. And it lays bare "an absence of consensus among Southerners on Confederate war aims" (p. xi).

Nelson's research is thorough, both in manuscript collections and in the most recent secondary literature, and his eye for revealing quotations is a source of satisfaction to the reader. He ably summarizes the many and changing aspects of domestic politics, North and South, and he provides a clear overview of the mission that Jefferson Davis sent to Canada in 1864 to facilitate the election of a United States president who would recognize Confederate independence. The portraits of major politicians seem brief but true and always employ appropriate quotations as evidence.

Nelson concludes that Davis's basic plan was intelligent and "reasonable" (p. 172), but the Confederate president receives low marks for implementation. The effort to affect Northern opinion scored some successes, but at times Davis worked at cross purposes with his Canadian agents. In regard to the internal challenge, Davis's performance was "seriously deficient" (p. 175), "haphazard . . . inconsistent . . . [and] belated" (p. 174). In general, these judgments are well supported.

Nelson asserts that Davis should have called for negotiations in August 1864, before the Democratic convention. His view places him in agreement with Alexander H. Stephens who believed that if an armistice could be achieved the war would never resume and Confederate independence would be assured. But Nelson also quotes Josiah Gorgas against Stephen's assumptions: "Cessation of hostilities would be fatal. Our armies would dissolve like frost before the rising sun" (p. 113). Moreover, Nelson makes clear that Stephens was primarily dedicated to states' rights and did not share Jefferson Davis's commitment to independence above all. Given that commitment, one wonders how much opportunity Davis had to maneuver in a Confederacy whose morale was "staggering" (p. 157). Stephens and many Southerners might have settled for less than independence. Whatever one's view, however, Nelson's analysis deftly spotlights the confusion that existed in the South over war aims.

This book fully deserved the Baruch Award that it won in 1976, and scholars will hope to see future works from the author.

PAUL D. ESCOTT
University of North Carolina,
Charlotte

C. VANN WOODWARD, editor. *Mary Chestnut's Civil War*. New Haven: Yale University Press. 1981. Pp. lviii, 886. \$29.95.

By his own admission, C. Vann Woodward was deeply torn in preparing this edition of what has long been known to American historians as *A Diary from Dixie*. His conflict arose from several complica-

tions in the original manuscript of more than two thousand copybook pages. The first and most troubling is that the two previous published versions (1904 and 1949) were drawn almost entirely from two versions that Mary Chesnut wrote in the 1880s. In short, no published edition of her writings, including the present one, was a diary in the sense that a historian would use that term, namely, a record written at the time of the events described, and not altered subsequently. As a consequence, Woodward has dropped the designation "diary" from his edition. A second complication for any editor, and Woodward wrestles with it manfully, is that portions of the original diary, which Chesnut used as the basis for her 1880s version, still exist. What to do with those additional one thousand pages of manuscript for the years 1861 and 1865 only? (To complicate the matter still further, Chesnut wrote an 1875 version, from which about four hundred pages survive.) Woodward has elected to insert certain passages from the original 1860s diary into the 1880s text and, occasionally, selections from the 1875 manuscript. Since he considers the 1880s version a work of art he is troubled by such intrusions, but as a historian he felt compelled to provide the reader with the additional and sometimes valuable material. Unfortunately, however, the reader still cannot examine the full 1860s diary; only those portions Woodward considered pertinent are included in this volume.

Woodward's decision to reprint in its entirety the second of the two 1880s versions but only selected passages from the 1860s diary is certainly understandable. Higher costs and greater bulk for an already expensive and stout volume were undoubtedly among the reasons, yet the decision is regrettable. For one thing, as he points out, a comparison of the changes between the 1860s and 1880s versions can be revealing. But we cannot learn all the differences since we do not have the full text of the original diary. Woodward tells us (pp. xxvii and 208n.) that in general there are few disparities in substance between the 1860s and 1880s manuscripts. Yet some items in the 1860s manuscript that Woodward thinks important enough to mention in his introduction are not in his printed text. Moreover, it is conceivable that a historian might have questions to ask about the two versions that Woodward had not considered. For example, do Chesnut's frequent references early on to the disparity between the resources and population of North and South occur in the 1860s diary as well as in the 1880s text? Was Negro dialect used in the 1860s manuscript as it is in the later one? And even Woodward notices that racial slurs against blacks appeared in the 1880s version that were absent in the 1860s journal. Bell Wiley in his comparison of the two versions in *Confederate Women* (1975) also notices discrepancies that are revealing.

Although this edition of Mary Chesnut's depiction of the rulers of the Confederacy is not ideal, it comes so much closer than any previous one that all others are now obsolete. It contains, for instance, large portions of the 1880s version that had been omitted by Ben Ames Williams in *A Diary from Dixie* (1949). In fact, the amount and quality of scholarly labor that went into this work is truly admirable. I estimate from the fifty-page index that perhaps as many as four thousand persons, places, and events are mentioned in the book, almost all of which are identified by the editor. Even the many quotations from Chesnut's wide reading in American and European literature and history are identified—and corrected, when necessary. Only some letters from relatives or friends, originally included by Chesnut, and some unintelligible or trivial references to social events have been excluded. Even then, the meticulous editor informs the reader in a footnote. On top of that scrupulous concern, we are also provided with a thoughtful and informed four-part introduction, which includes a sketch of Chesnut's life and personality and an explanation of the complex history of the manuscript, the previous editions, and the editorial policies that have been followed in this one.

There is not space here, nor is it necessary given the acquaintance historians have had with Chesnut's work from earlier editions, to attempt to characterize the contents. Suffice to say that my estimation of the value of the book and of Mary Chesnut is lower than Woodward's. If this is a work of art, as he, Edmund Wilson, and other commentators have contended, then it is a rather disjointed and poorly organized example. That there is hardly a page of this edition without at least one explanatory footnote—most pages have three or four—is at once a measure of Woodward's scholarly industry and of the elliptical and allusive nature of the text. There can be no doubt that it provides valuable insight into the doings and attitudes of the highest level of Confederate society. But that is also its limitation. The social stage is so narrow and the level of social and political criticism so shallow that the book is social history only in a restricted sense. Few persons outside the families of the leaders of Confederate society appear in these pages; even the slaves are seen only as they relate to their mistresses and masters. None of the characters in the book is really developed, and the number of people mentioned is so great that the stage seems confused as well as crowded. Moreover, Mary Chesnut seems to me pretentious, even snobbish, rather than critical or ironic. She parades her broad reading rather than reflecting on it. Her judgment of others is frequently critical, it is true, but her unflagging defense of Jefferson Davis is without self-doubt or questioning. And her well-known antislavery sentiments are ac-

companied by expressions of obvious racism. Occasionally, she actually strengthened the racism in the 1880s version over the 1860s journal. Although she is less vitriolic about Lincoln than some of her friends, she evidently had no conception of why the North fought the war other than from a deep and almost unreasoning hatred of the South.

Yet even a reader as unsympathetic as I felt something of what that ruling class was going through as its cause, which Mary Chesnut never did believe had much of a chance for success, went down to defeat. The dozens of conversations, diverse personalities, and personal observations lavishly reported here do in the end work their influence upon the reader. One feels the experience of defeat as well as comprehending it. In that sense, perhaps, the book does come close to a work of art even if hereafter it must be something less than a primary source of history.

CARL N. DEGLER
Stanford University

JANET SHARP HERMANN. *The Pursuit of a Dream*. New York: Oxford University Press. 1981. Pp. xi, 290. \$17.50.

This is a story about idealism and irony in the nineteenth-century South. The story begins with Joseph Davis, brother of Jefferson Davis, who attempted to apply Owenite principles at his plantation at Davis Bend on the Mississippi River below Vicksburg. Davis entrusted his slaves with self-government, provided well for their physical needs, and promoted their self-improvement with rewards and advancement. His policies brought profits and harmony to Davis Bend, but it did not challenge the institution of slavery. The slaves remained slaves in Davis's utopia. The Civil War revealed the limits of Davis's paternalistic vision, for when Davis fled his plantation to escape the Union army, the slaves stayed behind to welcome their "liberators." The blacks quickly learned, however, that the liberators wanted to impose their own paternalistic order on the "abandoned" plantations. Rather than work as wage laborers or tenants on plantations leased to Northern whites, the former slaves wanted to lease the land directly for themselves. The white authorities acceded to their demands but balked at letting the blacks run the cotton gin. Northern reformers did not think the blacks capable of operating a business by themselves; their version of black Reconstruction had its own paternalistic strictures.

The blacks' dream of economic and social independence seemingly prevailed over white dreams when Joseph Davis regained his property in 1865 and then sold it to Benjamin Montgomery, his former slave who had amassed a small fortune

running a plantation store at Davis Bend before the war. Montgomery leased the lands to black families, but he also introduced his own paternalism to Davis Bend. His family assumed the manners and lifestyle of the white planter class, while he continued the black lessees in a pattern of tenant dependence on his store and credit. At Davis Bend Montgomery planted the seeds of Booker T. Washington's strategy of accommodation and self-help. The Montgomerys courted the goodwill of local whites, discouraged black involvement in politics, and prescribed economic self-help as the surest tonic for racial harmony and progress. Their policies won them the sufferance and even support of whites to continue the experiment at Davis Bend.

The dream faltered in the late 1870s. The deaths of Joseph Davis and Benjamin Montgomery, the decline in world demand for cotton, the ravages of crop pests and floods, the losses from ill-advised investments in mercantile pursuits, the challenges of Jefferson Davis to his brother's will, and the continued inability of the Montgomerys to meet the mortgage payments proved too much for the black dreamers. In 1881 the plantation was sold at auction. Isaiah Montgomery, one of Benjamin's sons, attempted to revive his father's dream of an all-black community by founding the black town of Mound Bayou, but despite a brief prosperity it succumbed to natural and economic forces in the 1920s. The dreams at Davis Bend survived only as memories.

Janet Sharp Hermann—a gifted writer, sensitive to the nuances of human nature and the ironies of Southern history—gives a full, richly textured account of the ideals, wills, and vagaries that governed Davis Bend for over a century. Still, she could have done more in this book. Her intense focus on Davis and Montgomery obscures the rest of the occupants of Davis Bend; indeed, she relates very little about the black slaves and tenants there or how they felt about their condition under Davis and Montgomery. Although finely wrought, her portrait of Montgomery lacks soul. Hermann does not discover the wellspring of Montgomery's motives, his continued deference to Davis even when he had the resources to break free of the old ties. Like her subjects, she is reluctant to cross the river to compare Davis Bend with social experiments elsewhere in the Mississippi Valley, much less in the South. Apparently the physical isolation of the Davis Bend peninsula, which made dreams of utopia so inviting and plausible, narrowed Hermann's historical vision by keeping her too long on the plantation.

All this said, Hermann has given us an important, even a wonderful, book. She compels us to discard the old stereotypes of white oppressors and black victims by showing that within the boundaries of a racist society there was still room for black and white to treat one another with respect and even for the

black to gain some independence and prosperity. Blacks at Davis Bend did not wholly realize the promise of the American dream, of course, but for a time at least they got a chance to try. Believers in such dreams will be heartened by the generosity, common sense, and mutual respect that bound Davis and Montgomery together. Others will wonder if the failure of the experiments at Davis Bend should not call into question the assumptions undergirding the American dream and its applicability to the poor and minorities in American society. Perhaps it is time for a new dream.

RANDALL M. MILLER
Saint Joseph's University,
Philadelphia

CHARLES REAGAN WILSON. *Baptized in Blood: The Religion of the Lost Cause, 1865–1920*. Athens: University of Georgia Press. 1980. Pp. 256. \$19.95.

This interesting and valuable study breaks new ground in Reconstruction and New South history. It breaks new ground because Charles Reagan Wilson approaches his subject with a relatively new and untested concept—civil religion. This book is interesting and valuable because the theory of civil religion illuminates how Southerners coped with defeat in postbellum America.

The dream of a separate Southern nation ended after 1865, but students of Southern history know that the dream of a separate cultural identity did not die. The afterlife of a politically dead Confederacy is the subject matter of this book. The well-documented and freshly interpreted conclusion of this study is that while the Southern political nation was never resurrected it did live on as a powerful, sacred presence. How the South maintained its separate cultural identity and at the same time came to link a sense of Southern destiny with the Northern sense of American mission, culminating in the 1890s through the end of World War I, is a question well answered in this volume.

A chief interest of this book for me is Wilson's grasp of the central role of religion in the history of the "Lost Cause" attitude of Southerners. The title of the book is taken from a recurring phrase in the Confederate religious lexicon, "baptism in blood." This evocative phrase was the keynote theme of distinguished Presbyterian theologian James H. Thornwell in his sermon, "Our National Sins," preached on November 21, 1860. Calling for secession, Thornwell allowed that "our path to victory may be through a baptism of blood" (p. 5). Wilson convincingly shows that this powerful phrase became the ongoing interpretive theme that gave transcendent meaning to the political resolution of the sectional conflict and sustained the defeated

Southerner's sense of meaning, identity, and special destiny in the face of a lost cause.

The unique feature of this book is Wilson's use of the concept of civil religion. He argues that in America there is not one civil religion, but two. There is a Northern and Southern version. The Southern version is the religion of "Lost Cause" engendered out of defeat, poverty, confusion, social disorganization, and a profound identity crisis. Using primary sources—speeches, sermons, editorials, articles—from 1865 through 1920, Wilson shows that a blend of evangelical Protestant religion and new cultural Southern civil religion emerged to help Southerners cope with loss. By constantly memorializing dead Confederate soldiers and the death of the Confederate cause in sacred ceremonies, voluntary associations in the South—including evangelical Protestant churches—created a new "cult of the dead." By remembering the holy cause and mythologizing the past, as a means of symbolically overcoming their history, Southerners made a religion out of their history, according to Wilson.

Employing a functional definition of religion that draws deeply from the works of cultural anthropologists Anthony F. C. Wallace and Clifford Geertz, Wilson compellingly shows that the religion of the "Lost Cause" was an authentic religious "revitalization movement" expressed in "symbols, myth, ritual, theology, and organization, all directed toward meeting the profound concerns of postwar Southerners" (p. 11).

What makes this volume significant is both the demonstrated usefulness of the theory of civil religion in the hands of a historian and the fresh substantive contribution to the history of the South's tragic experience.

DONALD G. JONES
Drew University

JOEL WILLIAMSON. *New People: Miscegenation and Mulattoes in the United States*. New York: Free Press. 1980. Pp. xvi, 221. \$16.95.

Joel Williamson has courageously undertaken to write the first general history of mulattoes in the United States. Why has it taken so long? Perhaps because color-consciousness has been perceived as divisive within an Afro-American community whose civil rights and group interests are best served by black solidarity. Williamson has not been totally alone in his interest, and much of his story relies on piecemeal, local, and comparative work by James Hugo Johnson, Carl Degler, David Rankin, August Meier, David L. Lewis, John G. Mencke, Edward B. Reuter, and Laurence Glasco. Williamson seeks to combine this secondary literature with demography, black literature, and his own imagination to con-

struct a general historical development of his subject.

One of the book's major generalizations is that the upper South had a mild slavery but a harsh one-drop rule toward those of mixed race, whereas the lower South had a harsher slavery but a paradoxical leniency toward the relatively few free mulattoes. After 1850 the upper and lower white South joined in intolerance, and in the same period mulattoes and blacks "firmly cemented" an alliance and affinity that gradually grew stronger over the next century. Vestiges of preference for lightness within Afro-America lingered from emancipation to the 1920s, when physically and culturally Afro-Americans fused into a "new" people. Williamson sees both the Harlem Renaissance and the civil rights movement as stages in the steady darkening of "black America," achieving a biracial society not by white dictate but by black-brown embracement of blackness. The book ends with the assumption that miscegenation and intermarriage are, statistically speaking, disappearing, that a relatively monolithic black America coexists with a white one, separate but equal.

There is much room for argument here, and the reviewer hopes he has stated the generalizations fairly. To turn to specific mooted points: Is "mulatto" as a term to mean any person with a visible admixture of white and black sufficiently descriptive? The *degree* of admixture was often all important. Is it possible to rely on amateur census enumerators to accurately report skin color in the dim light of country cabins? During the Civil War and Reconstruction did octoroon and black, freeman and freedman, achieve solidarity, or is the careful conclusion of David Rankin to the contrary more to be trusted? If the Harlem Renaissance was a clarion call to black solidarity, where did Jean Toomer go? Is the author's vision of the present and future a true one, that out of the struggles of the 1960s what we have is the end of miscegenation and intermarriage and, within Afro-America, the collapse of all color and class distinctions into the clenched fist of black solidarity? Do we not still need a more sophisticated analysis of the full range of color identity and one that will also consider class structure in the analysis of the Afro-American minority?

LOUIS R. HARLAN
*University of Maryland,
College Park*

JEROME MUSHKAT. *The Reconstruction of the New York Democracy, 1861-1874*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1981. Pp. 328. \$25.00.

Jerome Mushkat, author of the lengthy *Tammany: The Evolution of a Political Machine, 1789-1865*, here

studies the Democratic party in New York State and City from its defeats and confusion during the secession crisis and early Civil War through its victories and reconstruction in the midterm elections of 1874. By far the bulk of his work is devoted to the postwar years. Mushkat focuses on the words and actions of party and factional leaders and newspapermen, largely ignoring the behavior of groups within the electorate. Mushkat claims that in reconstructing their party, Democratic leaders drew on the legacies of Jefferson and especially Jackson; that by 1874 they "proved that they had kept faith with longtime ideological commitments and had detailed programs for social justice" (p. 11); and that their accomplishments would endure into the twentieth century.

Although Mushkat offers detailed coverage of the Democracy and its factions and illuminates the Republicans and their cliques as well, he makes only a limited contribution to our understanding of party politics. To begin with, he fails to provide a conceptual framework for his close, year-by-year study. He might have made clear from the beginning that although the Republicans dominated nationally during this, the initial phase of the third electoral-political system, the Democrats remained competitive in New York State (carrying it in seven of fifteen years from 1860 through 1874) and claimed majority standing in New York City (winning it in all but one national or statewide election over the same years). To be sure, factionalism and scandals, which Mushkat treats, sometimes denied the Democracy victory in New York City and embarrassed it statewide, but by 1867, rather than 1874, the party appears to have gained a narrow edge in the Empire State. Mushkat shows no interest in determining whether the party's coalitional base shifted during the 1860s and early 1870s. He is unable to link the Democratic party's ideology to its triumphs and defeats at the polls. Further, Mushkat's failure to discern political patterns places him at the mercy of the manuscript and newspaper sources in which he immerses himself; he seemingly accepts at face value every statement by politicians and editors with regard to the ebb and flow of rival party fortunes. In thinking about politics in New York State, Mushkat would have profited from a careful reading of fine doctoral dissertations by Albert C. E. Parker (1975) and Phyllis F. Field (1974).

Finally, *The Reconstruction of the New York Democracy* is badly written. It should not have been published without extensive revision by the author and careful editing by the Fairleigh Dickinson University Press. Instead, the publisher contributed a number of typographical errors for good measure.

SAMUEL T. MCSEVENEY
Vanderbilt University

BARBARA LESLIE EPSTEIN. *The Politics of Domesticity: Women, Evangelism, and Temperance in Nineteenth-Century America*. Middletown, Conn.: Wesleyan University Press; distributed by Columbia University Press, New York. 1981. Pp. 188. \$17.95.

Barbara Leslie Epstein has written a thoughtful and lucid analysis of nineteenth-century culture. Finding that culture to have been shaped largely by antagonism between men and women, Epstein traces the development of sex-gender conflict through two movements of the period: evangelicalism and temperance. It is her contention that both these outpourings of female enthusiasm and energy were related to each other, with temperance transforming religious excitement into a secular set of demands.

The book compares women's and men's experiences in the two major periods of religious revivals in New England: the Great Awakening of 1740–44 and the revivals of the first four decades of the nineteenth century. Epstein points out that in the earlier revivals "men and women followed the same patterns of conversion, spoke of the same kinds of sins and described their experiences in similar language" (p. 14). Those of the nineteenth century, however, contained elements of female hostility to men, and Epstein sees this as the beginnings of a female culture.

Her use of both published and unpublished conversion narratives to reveal underlying gender antagonism is sensitive and compelling. Hatred, anger at God, and a desire to rebel against Him were common themes in these narratives that Epstein, correctly, I think, reads as animosity to males.

Less convincing, however, is her use of stories from popular religious journals. To be sure, these also exhibit a fair amount of female determination, a frequent tale being that of the persistent wife or daughter bringing the recalcitrant man into the church. Nevertheless, a broad sampling of female fiction from popular secular journals discloses quite the same assertiveness and even anger and calls into question Epstein's belief that "only in the name of something as deeply held and long established as orthodox religious belief could women bring themselves to challenge the supremacy of their husbands and fathers" (p. 62).

During the antebellum period women expressed their resentment over their subordinate role through illness, writing, and support of reform organizations such as the Female Moral Reform Society that openly and stubbornly challenged male authority.

While more attention to other manifestations of female anger would have deepened her thesis, the book, in general, is rich and provocative. Epstein's

explanation of the ways in which women's involvement in evangelicalism and temperance affected their views on the family and authority is fresh and illuminating, and the brief historical descriptions she provides for each movement is useful.

Missing, however, from Epstein's analysis is a clear sense of the relationship between evangelicalism and temperance. How did aggression toward men and male values that presumably led evangelical women to associate femaleness with piety become translated into the secular terms of temperance? How exactly was animosity transmitted from one generation of women to the next? That "the temperance movement was made up . . . of the same kind of people as those that shaped evangelical religion, people of the same class and ethnic background" does not tell us enough (p. 89). Who were the women of the temperance movement? What were their life experiences? It is difficult to imagine that the women who banded together under the banner of home protection were the same youthful group Epstein identifies as the nineteenth-century converts, fifteen to twenty-five years old, married or single, and without children. In short, what accounted for the continuities and discontinuities of the two movements?

Yet, while the link between evangelicalism and temperance is too vague for this reviewer's taste, *The Politics of Domesticity* has much that is new, interesting, and important.

BARBARA J. BERG
Marymount Manhattan College

RUTH BORDIN. *Woman and Temperance: The Quest for Power and Liberty, 1873–1900*. (American Civilization.) Philadelphia: Temple University Press. 1981. Pp. xviii, 221. \$17.50.

Here, at last, is a basic overview of the Woman's Christian Temperance Union, an especially welcome enterprise because a general treatment has been so long absent. As the fullest description of the organization on record, it is an essential tool for scholars of reform and women. While Bordin's book has been published simultaneously with Barbara Leslie Epstein's *Politics of Domesticity*, and both explore the origins and development of the WCTU from the Woman's Crusade to the end of the Frances Willard presidency, the two works address separate sets of concerns. While Epstein emphasizes the relationship of the WCTU to social movements around it, Bordin stresses internal dynamics, including leadership, tactics, programs, publications, the founding of local branches, charity work, and political endeavors.

Bordin provides a detailed and sympathetic look

at the era's largest organization of women, from its emergence in 1873 until 1900, just after Willard's death. She dissects the ambitious program that encompassed such diverse issues as prison reform, children, labor, social purity, and women's suffrage. Thus, the WCTU embraced elements that appealed to conservative church women and to suffragists and socialists as well. The appendix affirms the earlier studies that indicate that the bulk of membership consisted of white, well-educated, economically prosperous, native-born Protestants of Anglo-Saxon ancestry (p. 160). Nevertheless, this study lauds the political skills of Willard for fashioning a broad platform that also held appeal for many women of other racial and ethnic groups. In proving this, Bordin pays close attention to Southern branches of the WCTU and discusses the inclusion of black, native American, and immigrant women, and the discomfort felt with men, Chinese, and Catholics. Yet this is in no sense a biography of Willard, and the author is more attentive to her predecessor, Annie Wittenmyer, than earlier chroniclers of temperance have been, stressing the continuity of aspirations.

It is of no little importance that this author asks, "But is this feminism?" Bordin's answer is yes, claiming the WCTU's "Do Everything Policy" began as domestic feminism, aiming at elevating woman's position as the homemaker. Soon, however, the links between woman's vulnerability to a drunken husband and her low social status politicized members. "It ended with the Union leadership's commitment to revolutionary changes in the structure and organization of the economic system" and a large portion of the following "committed to militant feminism, equal rights and full participation in the political process" (p. 116).

Occasionally the author lapses into minutiae, providing for example excruciating detail regarding the financing of the Temple Building in Chicago when her energy might have been better spent on local case studies to complement her national focus. One might also hope for a portrait that is bolder about situating the WCTU in the context of the woman's movement in which it operated. Bordin does remind us that the WCTU was crucial in the passage of suffrage in Illinois, Iowa, and Washington Territory and that California suffragists begged WCTU members to hide their identities lest brewers squelch their campaign. But beyond this, she minimizes the cooperation among the multitudes of women's organizations of the day. In stressing that settlement houses stole WCTU issues while the less political General Federation of Women's Clubs and more elitist Association of Collegiate Alumnae stole its members, she obscures an impressive collaboration that built massive influence. The author need not champion the WCTU at the expense of women's

political, professional, social, charity, labor, religious, and literary associations, for it served as a model for most of them. Here is a major first step toward unraveling the complexity of their interaction.

KAREN J. BLAIR
University of Washington

JAMES A. KEHL. *Boss Rule in the Gilded Age: Matt Quay of Pennsylvania*. Pittsburgh: University of Pittsburgh Press. 1981. Pp. xx, 295. \$24.95.

The political career of Pennsylvania's Matthew Quay, (1833–1904), well merits James A. Kehl's excellent biography. Active in Republican party management and manipulation for nearly a half century, Quay was one of the shrewdest and most effective party bosses our political system has produced. Among his achievements were election to the United States Senate in 1887, 1893, and 1901; member and chairman of the Republican National Committee, 1888; and dominant figure in his state party's triumph in every congressional and presidential election from 1877 to 1904. All these accomplishments were important in those years, when the party organization was so pervasive and powerful that the word Republican was almost synonymous with the word government.

It was a rewarding era for political bosses. Blessed with sufficient brains, a cool head, untoward love of power, and a "stomach" for unsavory endeavor, Quay and such men as Tom Platt of neighboring New York State were able with relative ease to attain great power. They had patronage jobs to dispense, funds in state treasuries to control, high tariff rates to secure for "fat cat" industrialists and their high-wage work forces, and nominating conventions and elections to manipulate.

Pennsylvania Republicanism, because of its traditional low level reform zeal, was especially conducive to static bossism. The slavery question, which in the 1850s provided great impetus to the birth of the party, received lukewarm attention among most Pennsylvania political leaders. This irresolute attitude also carried over into the Reconstruction era. Uninspired, nonissue-oriented, power-conscious, opportunistic types such as Simon and J. Donald Cameron, Samuel J. Randall, and Quay handily obtained and retained control of the party, while in Massachusetts, as Kehl writes, ideological Republicanism advanced such persons as Charles Sumner, George Frisbie Hoar, and Henry Cabot Lodge.

Quay demonstrated no genuine interest in government policies, except somewhat mysteriously in American Indian matters. Because it was politically essential to do so, Quay helped steer high tariff

measures to passage but showed no interest in the measures themselves. His concern regarding tariff rates pleased Pennsylvania steel producers and hence assured Pennsylvania's role as the "financial capital of the Republican party, contributing much more—perhaps five times more—than New York to the party's coffers" (p. 99). As Kehl points out, critics could not attack Quay's program as a member of the Senate, because he had none.

Kehl deserves high praise for this biography. It is well written and based on extensive research, including use of hitherto unavailable personal papers of Quay. Kehl's account is enviably sophisticated, reflecting his disapproval of Quay's unfortunate character traits without engaging in old-fashioned muckraking, or spinning tiresome tales of corruption as if they were humor-laden.

HORACE SAMUEL MERRILL
University of Maryland

JEANNE MCHUGH. *Alexander Holley and the Makers of Steel*. (Johns Hopkins Studies in the History of Technology, new series, number 4.) Baltimore: Johns Hopkins University Press. 1980. Pp. xiv, 402. \$27.50.

Alexander Holley, who has been properly called "the father of modern American steel manufacture," has at last been given the biographical attention that he has long deserved. As writer, reporter, editor, mechanical draftsman, inventor, engineer, educator, and publicist, he is as close to being a Renaissance figure as late nineteenth-century industrial America could produce. In Jeanne McHugh, who for many years served as technical librarian and assistant to the vice president of research and technology at the American Iron and Steel Institute, Holley has found a biographer who can deal effectively with the central theme of his life, the development of steel manufacturing in this country. McHugh writes with a technical knowledge of her subject that would pass the scrutiny of Holley himself and with a clarity that makes understandable the complex process of steel making to the most uninformed and general of readers.

One should be prepared to take the second half of the title of this book seriously—as seriously as McHugh herself does, for this is more than a biography of a single man. This is prosopography, the collective biography of the steel makers, both European and American, whose technological innovations in one area of industry created our modern world. Beginning with chapter 6 and continuing for the next hundred pages, Holley's name is scarcely mentioned as McHugh relates the story of Henry Bessemer, his serendipitous discovery of the process that was to immortalize him, and the resulting

controversies with the American, William Kelly, and the Scot, Robert Mushet. McHugh deals with these famous controversies with an objectivity and fairness that is too often lacking in other histories of the steel industry. If Bessemer emerges as a little more petty and mean-spirited than his supporters would like, and if Kelly appears a little more knowledgeable, a little less of the ignorant country tinkerer than his detractors would admit, it is because McHugh has the evidence for these unbiased evaluations.

Important as these other biographies are, however, it is Holley who is the real protagonist of the story. Holder of ten major patents related to the Bessemer process, designer and builder of all but one of the first eleven Bessemer plants in the United States, Holley could claim without boasting that by 1880 the steel output in the United States from a pair of vessels was twice that of English converters because of "the superior arrangement and details of plant lay-out." And for that superiority of plant design, Holley was mainly responsible.

The research that has gone into McHugh's study is impressive. She can be faulted in this reviewer's opinion in only one instance. She gives too much credence to James Bridge's *The Inside History of the Carnegie Steel Company*. Bridge's book is a valuable source containing documents that are unavailable elsewhere. But as Andrew Carnegie's disgruntled former secretary who had tried unsuccessfully to blackmail his former employer, Bridge wrote his book in order to discredit Carnegie, and his highly biased interpretation of certain events relating to the history of Carnegie Steel needs to be treated with a greater chariness than McHugh gives.

Aside from Carnegie, there are few villains in this book, but there are a great many heroes: the two Fritz brothers, Göran Göransson, Robert Mushet, Captain Bill Jones, Sydney Gilchrist Thomas, and with qualifications, Henry Bessemer. But as McHugh makes abundantly clear, in this Augustan age of steel, "the noblest Roman of them all" was Alexander Lyman Holley. He graced his world with a generosity, a genius, and, most importantly, with a set of standards and values that the so-called Age of Enterprise sorely needed.

JOSEPH F. WALL
Grinnell College

THEODORE HERSHBERG, editor. *Philadelphia: Work, Space, Family, and Group Experience in the Nineteenth Century*. New York: Oxford University Press. 1981. Pp. xviii, 525. \$29.95.

This collection of papers demands to be judged against the goals and assertions repeatedly made by its editor, Theodore Hershberg, who is also director

of the Philadelphia Social History Project, the richly supported organization that facilitated the research presented. As Hershberg frequently reminds us, the primary purpose of both the book and the Philadelphia Social History Project is to break the research bonds of "the discipline-dominated world of higher education" (p. v) and to get away from the compartmentalized and "hopelessly fragmented" (p. 28) manner in which the city and the processes associated with it have been studied. He leads us to believe that only by interdisciplinary efforts and the bringing together of different theories, concepts, methods, and techniques can we "put together the pieces of the urban puzzle" (p. 22) of the past and present.

The core of this book consists of two papers on manufacturing; three interrelated articles on the journey to work, industrial location, and residential patterns and mobility; three comments on the family economy and decisions as to who worked, who went to school, and who stayed at home; and four papers on the "group experience" of Irish immigrants and blacks. Unfortunately, when one groups these contributions what one sees is several individual "pieces" that are highly impressive, a few clusters of interlocking "pieces" that prove rather revealing, but no completed "puzzle" of Philadelphia and the life led by its inhabitants in the latter half of the nineteenth century.

There are several reasons why Hershberg and his interdisciplinary associates fail to provide an integral picture of the processes and relations linking the changing character of work, family life, and group experience in the space (and place) that was Philadelphia. One of these is the authors' almost total emphasis on the measurable in general, and the categories contained in the project's bountiful data base in particular. This emphasis on tangible explanatory variables is undeniably rewarding—but only up to a point. It results in the Philadelphians of the period all too often being treated in a "thingified" and fragmented manner, rather than as people who were thinking and feeling subjects as well as objects in society. It ignores palpable relations and structures, confines analysis to frozen images of what was, and thereby provides too little understanding of the constant becoming of process.

Even more importantly, Hershberg and his associates fail because they do not fully appreciate that the creation of an integrated picture requires more than interdisciplinary good intentions; it requires a flexible and holistic conceptual framework with which quantitative and qualitative empirical efforts may interact. Admittedly, the city is sometimes loosely conceptualized as "process," or a set of dynamic "interrelationships among environment, behavior, and group experience" (p. 28), and there is occasional reference both to the dialectical interplay between urban spatial form and "the larger

social and industrial structure" (p. 122) and to the city as an "opportunity structure" (p. 456). However, no one of these themes is either fully developed into a holistic conceptual framework or used as a consistent frame of reference from one paper to the next. Given their intent to write history "from the bottom up" (p. 37), and given the subtitle of their book, Hershberg and his associates might have gotten all the "pieces" to fit together if they had taken account of the theory of structuration (or the dialectics of practice and structure) as developed by Pierre Bourdieu, Roy Bhaskar, Anthony Giddens, and others and if they had conceptualized Philadelphia as a ceaseless temporal and spatial interweaving of the city's particular physical components, its individual inhabitants, and the *concrete practices* of its families and economic, ethnic, and religious institutions.

At least three of the individual "pieces" are worthy of mention because of their clear presentation of findings that run contrary to prevailing wisdom. Bruce Laurie and Mark Schmitz demonstrate that, as late as 1880, mechanization and the use of power was unevenly rather than universally spread among Philadelphia's manufacturing establishments, and that, with few exceptions, the city's industries lacked economies of scale. Stephanie Greenberg reveals that, as of 1880, industrial affiliation rather than ethnicity was the "primary organizing factor" (p. 225) in the formation of Philadelphia's white residential communities. Frank Furstenberg, Hershberg himself, and John Modell contend that the frequency of female-headed families among blacks in nineteenth-century Philadelphia was not the legacy of slavery but the consequence of urban and structural factors—high mortality rates, grossly imbalanced sex ratios, severely limited remarriage markets, low wages, and poor working and living conditions.

Because of these findings and a number of other sound, specialized contributions, this book is a most welcome contribution to the literature on American urban history. Those seeking something resembling Hershberg's promised total picture, however, will have to look elsewhere.

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JOHN DIZIKES. *Sportsmen and Gamesmen*. Boston: Houghton Mifflin. 1981. Pp. xiv, 350. \$15.00.

Having read an earlier version of one of John Dizikes's chapters, "P. T. Barnum: Games and Hoaxing" in the *Yale Review* (Spring 1978), I looked forward to reviewing his new book with great expectations. I was not disappointed.

The book describes "the gradual transformation

of the aristocratic sporting tradition into a popular one" (p. 4), with the aristocratic tradition represented by the "sportsman" and the popular one by the "gamesman." This same dichotomy could be characterized as old versus new, aristocratic versus democratic, English versus American, or rural versus urban. While recognizing, as others have, that nationalism, the growth of cities, industry, and new forms of communication and transportation are crucial in an explanation of the changes in American sport, Dizikes focuses on the careers of eleven men and one woman for his analysis of alterations within the sporting world. Like others before him, he attempts to explain the rationalization and professionalization of American sports. His analysis is unique, however, in that he has chosen a biographical approach. Dizikes deals with change in sport by contrasting individual types: the "sportsman," who tended to accept both the explicit rules of the game and the unwritten code of conduct and who showed great concern for honor at the expense of victory, and the "gamesman," who refused to recognize the existence of any code of conduct and emphasized objectivity and strategy in directing all efforts toward winning. It is the gamesman, then, who the author says "came to be the dominant sporting and cultural type as the nineteenth century wore on; his style influenced the style and values of other professions and other activities" (p. 40). It was also the gamesman who exhibited the "poker face" that "eventually became the face for all public occasions" (p. 41).

The book is divided into three major parts: "City and Country," "The Challenge to England," and "Looking Back and Looking Forward." In the first part, which deals primarily with the period from 1800 to 1850, Andrew Jackson and Henry William Herbert (a sporting writer known as Frank Forester), are used to represent the English sportsmen of the past. Colonel William Ransom Johnson is portrayed as the gamesman of the future, and William T. Porter (editor of the *Spirit of the Times*) appears as a transitional figure who began his career with the challenge of making horse racing more respectable and who became more of a gamesman in later life. In the second part, Dizikes uses four major American challenges to British sporting superiority to document the transition toward gamesmen attitudes. Here, attention centers on John Cox Stevens and the 1851 race of the yacht "America," Richard Ten Broeck and a series of horse races commencing in 1856, Paul Morphy and championship chess in the summer of 1858, and, finally, John C. Heenan and his 1860 boxing match against Tom Sayres. Adah Menken, the risqué theater star who married Heenan in 1859, is discussed with her husband and is the only woman to receive "top billing" in the chapter selections. The last part of the book is

devoted to Hiram Woodruff, who represented sporting honesty and was consequently "looking back," P. T. Barnum, identified as "the greatest of nineteenth-century gamesmen," and R. C. Schenck, a poker player who was "looking forward." Also in this final chapter, Dizikes relates some of the major points in the book to the modern style of sports as exemplified in football, baseball, basketball, and tennis.

Each chapter, then, covers a lot of topical and chronological ground, in some instances too much. At times, Dizikes tends to overstate his case to make a point. It could also be argued that sporting ideology did not suffer a complete transformation and that the gamesman is just an alternate type of sportsman. This would be even more plausible if Dizikes had considered nonprofessional sports and amateurs.

Overall, Dizikes displays broad knowledge of a subject that crosses several different sports, generations, and cultures. He makes good use of a wide range of primary sources and secondary literature but would have benefited by reading some of the sociology and philosophy of sport literature where attempts have been made to define, analyze, and determine the inherent characteristics of play, games, and sport. Despite a few shortcomings, Dizikes is an inspiring writer who approaches the subject with vigor and a strong sense of direction. His book admirably makes the connection between sport and society at large, and, in so doing, should provide interesting and thought-provoking reading for a wide range of American historians.

JACK W. BERRYMAN
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RANDALL BENNETT WOODS. *A Black Odyssey: John Lewis Waller and the Promise of American Life, 1878-1900*. Lawrence: Regents Press of Kansas. 1981. Pp. xvii, 254. \$20.00.

A Missouri slave until he was twelve years old, John Lewis Waller became a freedman in mid-1862 when his family settled into rural poverty in Iowa. Completing high school and then reading law in a Cedar Rapids law firm, young Waller was admitted to the bar. He opened a law office in Leavenworth, Kansas, which had the state's largest black population, settling there on the eve of the great Kansas exodus of 1879. Waller's early life contained the basic ingredients for one seeking Horatio Alger success. He had more pluck than luck, however, so like most who aspired to Horatio Alger values, black or white, he did not rise from rags to riches. But, unlike most other Alger aspirants, neither did he go from rags to rags.

Briefly owning a chain of barber shops, trying several times to sustain a law practice, and holding a

succession of editorships with the bevy of black newspapers in Kansas, he never succeeded as a member of either the black professional or business elite. Waller was a fiercely loyal Republican who fought for civil rights and for getting blacks to vote Republican. In the 1880s he received only minor political appointments for his efforts (for example, as overseer of the broom factory at the state school for the blind). His failure to procure patronage, Randall Bennett Woods argues, was because of increasing racism and the decreasing impact of the black vote on the major parties. Only when the Democrats and Populists became a fusion threat to the Republicans did Waller achieve significant recognition, and in 1891 he was appointed by President Benjamin Harrison as United States consul to Madagascar. Two years later, after President Grover Cleveland had replaced him with a white Georgia Democrat, Queen Ranavalona conceded to Waller 150,000 acres of rubber and timber land to attract investors and black American settlers. France's imperialist ambitions, however, destroyed his hopes to develop "Wallerland": the French abrogated all past concessions, found Waller guilty of conspiracy, and shipped him to a Paris prison for twenty years of hard labor. Cleveland, pressured at home by jingoists and blacks, succeeded in freeing Waller (who had languished in jail for ten months) by conceding Waller's guilt.

Waller's continuing desire to help develop a new American empire was heightened by the Spanish-American War and his appointment as captain in a black regiment. Again his dream of black overseas settlement was stymied when Republican officials in Cuba withheld black patronage. By 1900 Waller abandoned the Republicans and assailed American imperialism. His last appointment, in the New York customs house, was a long way from placing him in the black vanguard of the new American empire.

Woods's book is strongest where primary sources are richest: during Waller's Madagascar adventures. So taken, however, by Waller's rhetoric for material gain and self-help, the author fails to explore sufficiently the relationship between black economic aspirations and political patronage. Waller was among those striving, middle-class blacks who sounded like Horatio Alger but sought political patronage as the vehicle for upward mobility. This biography expands our knowledge about black Kansans recently depicted in Nell Irvin Painter's *Exodus: Black Migration to Kansas After Reconstruction*, concentrating upon neither the masses nor the best known black leaders.

TILDEN G. EDELSTEIN
Rutgers University

ALFRED A. MOSS, JR. *The American Negro Academy: Voice of the Talented Tenth*. Baton Rouge: Louisiana

State University Press. 1981. Pp. 327. Cloth \$30.00, paper \$12.95.

In this study, Alfred Moss has rendered an important service to the specialist in black history and culture. He documents and narrates a detailed history of the American Negro Academy, a body that has been often mentioned but rarely examined in the major treatments of early twentieth-century black history, such as August Meier's *Negro Thought in America* and S. P. Fullinwider's *Mind and Mood of Black America*. References to the academy appear regularly in the private correspondence and public statements of most major black spokesmen of this era, including W. E. B. Du Bois, Booker T. Washington, Alain Locke, James Weldon Johnson, and William Monroe Trotter. The academy had, it is evident, wide visibility in its day. This fact would seem to indicate that it played a substantial role in Afro-American cultural life during the period of its existence from 1897 to 1928.

Moss finds the opposite, however. After a thorough examination of the available files, papers, speeches, private correspondence, and minutes of meetings, he is forced to conclude that the American Negro Academy never achieved a meaningful involvement in either the group politics, cultural activities, or intellectual debates that occurred among black Americans at that time. This conclusion led Moss to observe that "the real wonder in the history of the ANA is that the organization was able to exist as long as it did" (p. 304). Several factors limited the academy's effort and led ultimately to its demise. With the total number of members restricted to forty and the induction of women strongly discouraged, the academy always had a very small and largely male membership. The twenty-two individuals who showed up for the 1919 annual meeting, for example, represented one of the larger gatherings. So few members, drawn largely from the ranks of clergy, educators, and lawyers, proved an insufficient base upon which to build, both financially and in terms of morale. Without adequate monetary support, the academy was unable to publish a journal or consistently confront the major social issues of the period. Twenty-two "occasional papers" appeared over the three decades that the academy functioned, but they were published infrequently and often were untimely in their concerns.

The leadership of the American Negro Academy also reflected the general problems of the organization. For differing reasons, those who filled the key post of president—Alexander Crummell, Du Bois, Archibald Grimke, John Cromwell, and Arthur Schomburg were unable to marshal the academy's resources or provide clear direction for its efforts. The Grimke presidency from 1903 to 1919 was one of stability and modest growth, but at the same time

demonstrated the provincial nature of the academy. A lawyer and author, Grimke represented the unusually large Washington, D.C. contingent that dominated the organization. In fact, the academy was in many ways a local institution. All its meetings, save the 1920 assembly held in Harlem, convened in Washington. Most of its leadership and a majority of its membership came from the District of Columbia, another indication of the academy's narrow support base.

All of these factors, as Moss carefully records, came together in the 1920s to bring about the academy's collapse. In his study of this development, he has clarified the role of the American Negro Academy and its limitations. He has imaginatively utilized an institutional failure to reveal the complexity and significance of an evolving black cultural ethos during those years.

RONALD M. JOHNSON
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MARIO T. GARCIA. *Desert Immigrants: The Mexicans of El Paso, 1880–1920*. (Yale Western Americana Series, number 32.) New Haven: Yale University Press. 1981. Pp. xii, 316. \$23.00.

This study traces the development of the Mexican immigrant community in El Paso, Texas, from 1880 through 1920. Using the local Spanish and English press, school system, and municipal records and a number of regional manuscript collections, Mario T. Garcia provides a lively depiction of the process of immigration as well as the working lives, schools, housing, culture, and politics of Mexicans in El Paso.

Consistent with other recent works in Chicano history, Garcia discerns a close link between Mexican immigration and the growth of industrial capitalism in the United States. In short, American business interests encouraged Mexican immigration in order to meet the needs of the Southwest's labor intensive extractive economy, which was dedicated to providing agricultural products and raw materials to the industrializing states of the Northeast and Midwest. Once in this country, Mexicans were relegated to a menial labor class and enjoyed little chance for social mobility due to racial discrimination and limited job opportunities in an immature economy. Probing city directories and manuscript censuses and borrowing the methodologies of Peter Knights and Stephen Thernstrom in their studies of nineteenth-century Boston, Garcia develops data that confirm the well-known observation that the Southwest was characterized by racially related social stratification at the turn of the century.

An outstanding chapter on Mexican schools in El Paso presents the freshest information on the Mexi-

can immigrant experience in the United States. Anglos in El Paso viewed public education as a means to economic and social control of the Mexican immigrant population. They provided Mexicans with a rudimentary segregated public education that had the dual purpose of preparing Mexican boys and girls for manual and domestic labor while exposing them to an Americanization curriculum designed to inspire their loyalty to the United States and its values.

The book is thinnest when attempting comparisons between the Mexican and the European immigrant experiences. For example, in the introduction, Garcia asserts that "El Paso symbolized to Mexicans what New York had represented to European immigrants: the opening to what they believed would be a better life" (p. 2). This represents the seed of an intriguing and sweeping observation that unfortunately never blooms in the pages that follow. From Garcia's description of El Paso one fails to get a sense that El Paso contained either the physical or emotional equivalent of Ellis Island and the Statue of Liberty. In fact, what Garcia correctly characterizes as Mexican immigrants' "adamant belief" (p. 6) that their stay in the United States was only temporary led to important differences between the Mexican and European immigrant experiences in this country.

Comparative immigration history was not the author's primary intent, however. As a solid study of the vital role Mexicans played in the development of a major Southwestern city, *Desert Immigrants* is a noteworthy addition to the increasingly sophisticated and growing body of Chicano history.

MARK REISLER

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ROBERT J. ROSENBAUM. *Mexicano Resistance in the Southwest: "The Sacred Right of Self-Preservation."* (Dan Danciger Publication Series.) Austin: University of Texas Press. 1981. Pp. xii, 241. \$14.95.

Robert J. Rosenbaum has provided the first book-length examination of *mexicano* resistance to the Americanization of the Southwest, but his study is narrower in time and space than its broad title implies. No more than 10 percent of the book discusses the twentieth century, and over half the text and all thirty pages of the appendixes treat New Mexico. In comparison, California and Texas are slighted, and Arizona is explicitly omitted.

Rosenbaum concentrates on violent forms of resistance, and makes admirable use of the distinctions and categories of social scientists. He argues that "all incidents of political violence [by *Mexicanos* in the Southwest] fall within the general category

that Eric Hobsbawm terms 'peasant' or 'primitive' rebellions" (p. 16). Within this general category, Rosenbaum identifies and analyzes five types of political violence, which are not mutually exclusive: border warfare, social banditry, community upheaval, skirmishing resistance, and coordinated rebellion.

Rosenbaum illustrates his first type, border warfare, with three well-known episodes from the Texas-Mexican border: the Cortina rebellion of 1859; the remarkable case of Gregorio Cortez, 1901-13; and the Plan of San Diego, 1915. He finds border warfare distinctive by virtue of its unusually high degree of violence in a region where proximity to Mexico "kept irredentist sentiment high" (p. 39). To demonstrate the second type of political violence, social banditry, Rosenbaum looks at California and the familiar cases of Tiburcio Vasquez and Joaquin Murrieta. To illustrate community upheaval, his third category, Rosenbaum uses two incidents in southern California in the 1850s as prime examples.

Rosenbaum's forays into California and Texas derive largely from published sources. When he moves into New Mexico to illustrate his fourth type of political violence, skirmishing resistance, the sources become richer and the narrative more detailed as he examines the struggle over the Maxwell land grant and the Lincoln County War in two full chapters. His most elaborate analysis is of the fifth type, coordinated rebellion, illustrated in three chapters. Here Rosenbaum examines the rise of a *mexicano* vigilante group in the 1880s, the White Caps, and its metamorphosis into a political party, the Partido del Pueblo Unido. He sees the Partido del Pueblo Unido as "a dramatic precursor to modes of self-preservation used by *mexicanos* in the twentieth century [United States]" (p. 145) and devotes a portion of his final chapter to what he terms "*mexicano* efforts at self-preservation through politics" in twentieth-century America. Rosenbaum is pessimistic about the potential of electoral politics as a vehicle for cultural self-preservation, and his conclusion that profound class and regional differences prevented Mexican-Americans from uniting in the nineteenth century applies yet today.

Mexicano Resistance often falls short of its author's ambitious conception. Some of Rosenbaum's generalizations outweigh the evidence; background information is often not tied clearly to the thrust of the argument; and the narrative potential of this fascinating subject never rises above a didactic style. By emphasizing political violence, Rosenbaum loses sight of what he explicitly terms "the central question of this study . . . how did *mexicanos* respond to the increasing pressures brought by Anglo American domination" (p. 7). Only a small if indeterminate percentage of *mexicanos* responded through violence. What of the others? Moreover, it may be

that Rosenbaum has pushed Hobsbawm's useful model too far by forcing Mexican frontiersmen into a "peasant" mold and by creating a "capitalist-peasant dichotomy" (p. 12) that did not exist. Finally, at what point does political violence cease to be "primitive"? That Rosenbaum's study provokes such a question is one measure of its success.

DAVID J. WEBER
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STEPHEN STEINBERG. *The Ethnic Myth: Race, Ethnicity, and Class in America*. New York: Atheneum. 1981. Pp. x, 277. \$14.95.

Two fundamental ideas about ethnicity in America inform this new book by Stephen Steinberg. The first is that the historical record overwhelmingly supports the assimilationist rather than the pluralist interpretation. The second is that ethnic group differences are rooted in socioeconomic rather than cultural variables. The author does not deny the potency of ethnicity; his purpose is rather to explore its historical and structural foundations.

The first part is a survey of pluralism in American history. He establishes its essentially negative character and shows that the essentially Anglo-Saxon core culture did not permit Indian, black, or immigrant cultures to thrive. Given their places near or at the bottom of the American class structure, racial and ethnic groups were compelled to assimilate in order to realize any economic and social mobility.

Steinberg builds on the foundation laid a half-century ago by Robert Park and the Chicago school of sociologists. He convincingly argues that much recent sociological interpretation of ethnicity is deeply flawed by a failure to place it in the context of long-range historical trends. He does not predict the imminent disappearance of ethnicity, yet the homogenizing powers of modern mass society are such that ethnic institutions are losing their traditional functions. When ethnicity is no longer integrated into the material circumstances of people's lives, writes Steinberg, atrophy is inevitable. Thus the recent resurgence of ethnic culture in America is merely another stage in the inexorable process of assimilation rather than a genuine revitalization.

The second part treats the role of social class in the formation of ethnic myths. Here Steinberg takes issue with writers who attribute ethnic group success or failure to cultural factors. The exceptional success of the Jews, for example, is not attributable to traditional group values, as Nathan Glazer has emphasized, but rather to variables such as occupation and literacy. Unusually high proportions of Jewish immigrants were urban skilled workers or were engaged in commerce while other groups were mostly of peasant stock. While Jewish culture placed

special emphasis on learning, Jewish literacy rates were not unusual for groups originating in urban industrial areas. Similarly, he argues that culture never holds people back from material success; the blame for that must always rest with the poverty associated with newcomer status. High crime rates, for example, are always associated with the most recent arrivals—first the Irish, then the Italians, later the blacks from the South, Chicanos, and Puerto Ricans. There is no "culture of poverty," as Oscar Lewis has taught; antisocial behavior is a response to economic disadvantage. In Steinberg's scheme of things, culture is never a cause; at most it functions as a conditioning agent on pre-existing class factors.

In this connection Steinberg disagrees with Daniel Moynihan, who has argued that the single most important problem among blacks in America is the deterioration of the Negro family due to the heritage of slavery. Steinberg finds this interpretation seductive because it puts the blame for black poverty on a racist society. Yet he warns against it because our sins and not the sins of our fathers are at the root of the problem. It is curious that Steinberg should assume such a presentist, normative stance after having pleaded so eloquently for an understanding of the historical context of ethnicity in America.

In the third part the author extends his class interpretation to ethnic conflict. He uses several historical examples, including the reconstruction of black servitude after the Civil War and the "Jewish problem" in American higher education, and finds that "ethnic conflict is often only a surface manifestation of a deeper conflict of an essentially social class character" (p. 170).

This book is well written and often compelling in its arguments. There is never any doubt about where the author stands; his adherence to a doctrinaire liberalism and an economic determinism is both open and unyielding. *The Ethnic Myth* has the character of a "tract for the times" intended more to persuade than to illuminate.

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ANTHONY J. KUZNIEWSKI. *Faith and Fatherland: The Polish Church War in Wisconsin, 1896–1918*. (Notre Dame Studies in American Catholicism, number 3.) Notre Dame: University of Notre Dame Press. 1980. Pp. xii, 171. \$12.95.

The traditional link between the Polish nationality and the Catholic religion was strained for many immigrants to the United States. This slender book recounts in an impartial way and for the first time in

English the unique conflict that developed between Poles in Wisconsin and the church authorities when the former demanded representation in the hierarchy befitting their growing numbers. The Poles constituted a relatively homogeneous group, for most of them had come from the Prussian partition of their divided fatherland, where they not only had been given more schooling than their compatriots in the other partitions but had also been accustomed to resist the efforts of German bishops to make them adopt the language and customs of their civil rulers. Analogously, the Catholic bishops of Wisconsin, who were all of German, Austrian, Swiss, or Luxembourg birth or descent, tried to Americanize the immigrants gradually while allowing them their "national," or ethnic, parishes. The movement to obtain recognition of the Polish presence on a higher level was led by Wenceslaus Kruszk, a priest, journalist, historian, and agitator, and his half-brother, Michael Kruszk, a layman, politician, and newspaper publisher. The acute phase of the struggle began when Sebastian Messmer (1847–1930) was promoted from bishop of Green Bay to archbishop of Milwaukee in 1903. Considering the Poles too separatist and foreign, he withstood the ever more strident demands and vituperative reproaches of the Kruszk brothers and their followers until 1913. Then, urged even by respectful Polish priests and fearful of losing disaffected Poles to the schismatic churches that were spreading in other states, he asked the Holy See to appoint a Polish-American priest, Edward Kozłowski, his auxiliary bishop. Two years later he had the first Polish-American bishop, Paul Rhode, who had been an auxiliary in Chicago since 1907, named the first Polish head of a diocese in the United States, namely, Green Bay. In the same year, however, Kozłowski died, and no other Pole was raised to the episcopal rank in the province (or in the country) during Messmer's lifetime, lest assimilation be retarded.

Kuzniewski narrates the Poles' campaign and the prelates' counteroffensive clearly and succinctly and summarizes the arguments presented in polemical publications and legal suits. When he did the research for the doctoral dissertation (at Harvard University) from which this book is derived, the ecclesiastical archives in Rome were still closed for his whole period but since then have been opened for the pontificate of Leo XIII. Presumably they contain further information on these controversies, for example, the reason why Cardinal Mieczysław Ledóchowski, prefect of the *Congregatio de propaganda fide* (which supervised the church in America until 1908), refused the request of Messmer's predecessor, Archbishop Frederick Katzer, for a Polish auxiliary in 1902. The author's judgment of Michael Kruszk, who also championed such causes as the

holding of parish property and the control of parish finances by laymen, seems to be more generous than is warranted by the bellicose leader's conduct. It is noteworthy that Michael always retained a greater popularity among the laity than Wenceslaus did among the clergy; most of the Polish priests, in fact, repudiated their obstreperous colleague's methods and supported Messmer.

This book will be useful to social as well as religious historians. It is illustrated with photographs and newspaper cartoons but has the notes inconveniently placed at the back.

ROBERT TRISCO

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FRANCIS M. CARROLL. *American Opinion and the Irish Question, 1919–23: A Study in Opinion and Policy*. New York: St. Martin's Press or Gill and Macmillan, Dublin. Pp. xi. 319. \$25.00.

As described by Francis M. Carroll, in 1910 the Irish parliamentary party's control of the balance of power at Westminster promised an early advent of Home Rule. Reflecting the strength of constitutional nationalism in Ireland, the United Irish League of America represented the vast majority of Irish-Americans, frustrating the revolutionary hopes of the Clan na Gael.

From 1912 to 1916 British politicians radicalized Irish nationalism on both sides of the Atlantic. In 1913 they rewarded the treasonable activities of Ulster Protestant fanatics by promising to exclude their territory from a Home Rule Ireland. In 1914 they failed to implement the Home Rule Bill. In 1915 they constructed a coalition cabinet including Unionist enemies of Irish nationalism. And in 1916 they transformed the leaders of the Easter Week rebellion into martyrs by slowly and sadistically executing them.

Because of "racial" or religious prejudices, a number of Anglo-Americans (Carroll inaccurately defines them as native Americans) despised Irish Catholics and their nationalism. Easter Week and the Anglo-Irish war (1919–21) intensified and spread this nativism that accused the Irish of pro-German and anti-American activities. However, many Anglo-Americans, including Republican and Democratic politicians, championed Irish freedom as a matter of justice or because it would remove a thorny issue from American politics. But they failed to influence significantly the British government's Irish policy because Westminster politicians were more sensitive to the Ulster Protestant mentality than to the sentiments of Irish or Irish-American nationalists or to the exigencies of the World War I Anglo-American alliance. And Woodrow Wilson was more concerned with Anglo-American ties and

the fate of the League of Nations than with Irish self-government.

According to Carroll, Irish-Americans did not reject Wilson in the 1916 election but after Versailles many of them were convinced that he had sacrificed self-determination principles to Anglophilia. They retaliated by joining the coalition that defeated the League in the Senate.

Although Irish-American nationalist opinion divided into pro- and anti-De Valera factions during the Anglo-Irish war, it contributed to the pressures that persuaded Britain into conceding Dominion status to twenty-six Irish counties. Most Irish-Americans accepted the Anglo-Irish Treaty (1921) as a base for an expanding Irish sovereignty and were disgusted by the 1922–23 civil war between Free Staters and Republicans. From 1922 until recent events in Northern Ireland, upwardly mobile Irish-Americans concentrated their energy and enthusiasms on American issues.

Although Carroll does not surprise the informed reader with new discoveries or brilliant interpretations, he organizes a great amount of interesting material into a coherent story. His thoroughly researched, lucidly written, well-structured, and thoughtfully presented book is an excellent source on the sometimes paranoid psychology of Irish-American nationalism, the genius that British politicians have for destroying Irish voices of reason and moderation, the close relationship between Anglo-Saxon and Anglo-American nativism, and, despite their obvious political talents, the inability of Irish-Americans to sever that bond.

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RICHARD E. WELCH, JR. *Response to Imperialism: The United States and the Philippine-American War, 1899–1902*. Chapel Hill: University of North Carolina Press. 1979. Pp. xvi, 215. \$17.50.

In the past few years there have been several studies written on Philippine-American relations. Most of them dealt with issues of imperial governance and whether the United States had some well-defined course of action for governing the Philippine Islands. Those monographs either plowed new ground or challenged old assumptions, and some did this better than others. This present study is not among that genre of colonial self-analysis. Richard E. Welch, Jr., is not a Filipinoist, although his credentials as a serious student of the Philippines are unchallenged. He is an American historian who has been very interested in the dynamics of the Philippine-American experience as seen primarily from this side of the Pacific Ocean, and he has written

extensively on the early years of the Philippine-American relationship.

It is over eighty years since Commodore George Dewey brought the United States East Asiatic Naval Squadron into Manila Bay. There are very few Americans and Filipinos alive who can recall the events of those days. Between 1899 and 1902 the American nation was deeply divided over the Philippine Islands. Welch has surveyed and evaluated every significant component of the great debate over the Philippine problem: the decision to go to the Philippines, the Philippine-American war (or Filipino insurrection as most Americans still call it), the reasons for annexation, the role of business and overseas economic expansion, the attitudes of labor to "cheap men," the issues of racism, the arguments of churches for expanded missionary opportunities, the military and atrocities, the politicians, the use of the media, and the place of patriotism versus the Anti-Imperialist Leagues. It is all there, and this book is an important contribution to the historical literature of the Philippine-American experience.

Welch researched the major libraries and manuscript collections in the United States and used hundreds of articles and monographs in the preparation of his study. The absence of Filipino sources contemporary to the events of the war is not detrimental since the theme of this monograph is the American response. It would be interesting to learn what the Filipinos understood about the United States and how Americans responded to the war, atrocities in the Philippines, and so forth. That particular study may have to be done by a Filipino scholar working in the Philippines.

Lastly, there are two things that, in my opinion, detract from what is an excellent examination of those important issues of eight decades ago. The first is Welch's effort to explain that the Philippine-American conflict was not a training ground for the Vietnamese war. No serious scholar believes that to be the case. He worries too much about polemicists who think they know about Vietnam and the Philippine-American war. The second is that Welch falls victim to his own erudite command of the English language. His editor should have chopped mercilessly or asked him to hold back. His scholarship should be understood by everyone. These are small distractions, however, that will not disturb those interested in the United States and the acquisition of the Philippine Islands.

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ROBERT C. HILDERBRAND. *Power and the People: Executive Management of Public Opinion in Foreign Affairs*,

1897–1921. (Supplementary Volumes to *The Papers of Woodrow Wilson*.) Chapel Hill: University of North Carolina Press. 1981. Pp. 262. \$19.00.

Public opinion is often assumed to play an important part in decision making, but the precise relation between opinion and policy formulation is difficult to measure. A number of scholars have made worthwhile contributions to our understanding of this process. To the list of such writers must be added Robert C. Hilderbrand, whose *Power and the People* examines presidential efforts to manage opinion in foreign policy from William McKinley through Woodrow Wilson.

Beginning with McKinley, presidents exhibited an increasing interest in influencing opinion. Several developments account for this heightened attention including technological innovations that transformed mass communications; a growing use of advertising and public relations techniques that could be applied to government as well as business; America's expanding role in world affairs; progressive ideology with "its emphasis on popular foundations of power" (p. 4); and the very process of modernization that changed the federal government and contributed to the "imperial presidency." As interest in popular attitudes increased, so did the president's attempts to guide and influence public understanding of international affairs. One result was a decline in the public's independence.

McKinley emerges from this study as a leader who guided opinion during the Cuban crisis by understatement and who generally was in command of events. Hilderbrand argues that Theodore Roosevelt actually played a secondary role to McKinley as "the originator of executive publicity" (p. 199). To be sure, the charismatic Roosevelt used the press more effectively, but in many ways he merely employed techniques already established by his predecessor. Not surprisingly, William Howard Taft appears as the least successful president in dealing with the public largely due to his personality and his misgivings about increasing presidential authority. Wilson, however, further enlarged the chief executive's influence by establishing regular press conferences in his first term and by creating during the Great War the enormously successful Committee on Public Information, the nation's first large-scale propaganda agency. By the time Wilson left office the White House had become the capital's most productive news source, and the executive's ability to manipulate opinion had been greatly enhanced.

Hilderbrand made wide use of primary sources, and his book is a solid contribution although not without shortcomings. One might challenge the assertion that "public opinion had little impact on foreign policy making before 1897" (p. 5). The treatment of the Wilson administration, which comprises about half the text, covers much familiar

terrain. And, there are occasional factual inaccuracies. Guy Stanton Ford, for example, was executive secretary of the American Historical Association and not president as the author maintains (p. 155).

These flaws do not overly detract from what is otherwise a fine study. Wisely, the author did not attempt to describe public opinion during the period, a task that would have been difficult if not impossible. Rather, he focused on the chief executive's increased awareness of his ability to influence opinion and in so doing tried to answer questions—too often neglected—about presidential understanding of popular perceptions. Have presidents sometimes ignored public attitudes in favor of more pressing considerations? Did presidents perceive opinion as a constant in foreign policy calculations, or could it be changed? Did presidents see the public as an impediment or an instrument of foreign policy? The work is perhaps most interesting in its discussion of progressivism and its relation to presidential efforts to direct opinion. Progressivism encouraged popular participation in government, but it also had faith in a strong president and in the application of good management to all administrative endeavors. Such beliefs helped increase the chief executive's control over opinion, Hilderbrand maintains, and served to produce a reversal of the progressive spirit. The public had little say in foreign policy decisions between 1897 and 1921.

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MARK K. BAUMAN. *Warren Akin Candler: The Conservative as Idealist*. Metuchen, N.J.: Scarecrow Press. 1981. Pp. x. 278. \$16.00.

Warren A. Candler (1857–1941), bishop of the Southern Methodist Church for thirty-six years, prolific Methodist apologist, and builder of Emory University, is a relatively obscure figure today, but during the first decades of this century the Georgia churchman had enormous influence and prestige. His long life encompassed the whole spectrum of ministerial activities: pastor, revivalist, educator, publicist, crusader for righteous causes, administrator, adviser to high and low alike. Candler could be in turn stodgy and bold, rigid and understanding, defender of orthodoxy and supporter of academic freedom, racist and supporter of black colleges and opponent of lynching, proud Southerner and critic of the region. The strong-willed, even authoritarian bishop was a keen judge of persons, a skilled fundraiser, a "facilitator" rather than a sophisticated theologian. He understood the attitudes and values of his parishioners and never ventured far beyond their views.

Such a person as Warren Candler presents difficulties for the biographer. Not adventurous enough a thinker or visionary enough a planner to be truly exciting, too conventional to elicit real admiration, too important a church leader to ignore, Candler merits better understanding in part because he reveals so much about the world he shared with his parishioners. Mark K. Bauman has performed his biographer's task quite adequately. Mining the voluminous Candler manuscripts at Emory, the thousands of editorials Candler wrote for scores of secular and denominational newspapers, and extensive other materials, including oral interviews, Bauman has provided a careful and detailed account of Candler's life. His childhood, education, and long career are covered in narrative fashion, and the development of Candler's views on a wide variety of subjects is outlined. On issue after issue the bishop's ideas are summarized, Bauman grouping many of Candler's key beliefs under the heading of "three pillars of civilization": family, church, and state. Candler's was a thoroughly conventional theology. He was a proper Southern Victorian gentleman of the old school, and when the twentieth century, particularly the post-World War I era, began to challenge his world view, Candler was unable to adjust and grow.

Increasingly after the early 1920s Candler fought a rearguard action to hold on to the old verities, and he was painfully out of step with modernity, which brought him ebbing influence, frustration, and defeat. Yet he remained true to his values to the very end, and Bauman—while often disapproving of Candler's theological stances—becomes grudgingly admiring of Candler the man: a steadfast friend, devoted husband and father, a thoroughly decent person doing right by his lights. As a consequence the biographer presents a well-rounded portrait of his subject, pointing out the contradictions and private personal actions that make all persons far more complex than the cardboard creatures they often present for view.

Considering the total man, Bauman finds Candler not only less doctrinaire and narrow-minded than his editorials and ecclesiastical decisions sometimes suggest but also a gentler, kinder, greater man. Bauman's work is at times poorly organized, his writing lacks grace, his analysis is seldom penetrating, but it suffices to rescue Warren A. Candler from undeserved obscurity and restore to him the stature he earned. For that reason the biography serves us well.

JOHN B. BOLES
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SEAN DENNIS CASHMAN. *Prohibition: The Lie of the Land*. New York: Free Press. 1981. Pp. xiv, 290. \$17.95.

"So, yet another book on prohibition?" This is not my question, but the author's (p. xii). Why, indeed, in view of Asbury, Merz, Sinclair, Dobyns, Kyvig, and others? Sean Dennis Cashman claims that it is necessary for two reasons: (1) "none is truly comprehensive" and (2) "much of the original evidence is disintegrating with age" (p. xii).

The result is a slightly bewildering potpourri of diverse information about the prohibition years gathered together rather appealingly in a book of modest length. Beginning with the post-Civil War era, Cashman traces the roots of prohibition to the passage of the Volstead Act, offering us the usual fare. Although he strives to make his interpretations unique, he mainly agrees with what most social historians have long since concluded: that prohibition was not foisted on the nation by a precipitous Congress, was not merely a by-product of women's suffrage, was not simply a rural victory over the city, and was not just an emergency measure to conserve food resources during the war. It was the complex interaction of all these factors—and more.

Once past this attempt at novelty, Cashman gives us relatively concise, sometimes strangely organized, but almost always sprightly written accounts of rum-running, the growth of speakeasies and the activities of their famous entertainers, the ineffectiveness of the Prohibition Unit, the antics of agents Izzie and Moe, Chicago gang warfare and the rise of Al Capone, Eliot Ness and the work of his "Untouchables," the impact of prohibition on the arts and movies, a summation of favorite cocktail recipes (what "comprehensive" book on prohibition could fail to include them!), and so on. In dealing with prohibition politics and with the final movement for repeal, Cashman again provides us with no surprises.

This book is more compilation than synthesis, analysis, or distillation. All the old saws, clichés, and hallmarks of the period are here: from Texas Guinan's brassy "Hello, suckers" to Franklin P. Adams's famous doggerel "Prohibition is an awful flop. We like it"; from Mae West's assertion to Cary Grant, "When I'm good I'm very good, but when I'm bad I'm better" to James Cagney's film death on the snow-covered steps of a church in *The Roaring Twenties*. All this is very pleasant, but it tends to become kaleidoscopic and disjointed. Readers often must supply the unity themselves. For example, the vocabulary of prohibition—"hot seat," "honky tonk," "stool pigeon," "alky," and so forth—is discussed at the end of a chapter on the rising Mafia, for no apparent reason. Some things jar. A comparison of the activities of the Mafia with those of the so-called "robber barons" of the late nineteenth century is both sophomoric and dated. It simply will not do to put Johnny Torrio and Andrew Carnegie in the same historical line-up. Indeed, the author (who is a lecturer in American history at Manchester

University, England) spends too much time and space on crime, gangsters, and the *Unione Siciliana*. Prohibition, after all, was not merely a Chicago and New York phenomenon.

Still, for the casual history buff who wants a book that brings together most of the facts, legends, villains, and heroes of this rapidly receding American scene, Cashman goes a long way to provide it. For historical scholars interested in new contributions to knowledge and in interpretations involving the subtleties of the prohibition movement, they will not find them here. For undergraduate students who wish an entertaining collateral reading experience, this book should be their "meat." Drink, too, if they decide to try some of the recipes.

ROBERT K. MURRAY

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JOHN S. HALLER, JR. *American Medicine in Transition, 1840-1914*. Urbana: University of Illinois Press. 1981. Pp. xii, 457. \$27.95.

American Medicine in Transition consists of eight essays dealing with various aspects of American medicine, all of which can stand quite well by themselves. John S. Haller, Jr., prefaces each of his topics with a short sketch of the historical background. For example, in the first essay, which deals with medical theory, he traces constitutional pathology, as expressed in the humoral theory, from earliest times to the present. Since humors affected the body in health and disease, diagnosis in the nineteenth century was based largely upon knowing the temperament, habits, and predispositions of the patient. As Haller shows, constitutional pathology survived into the twentieth century through criminal pathology, craniometry, and the concept of body types. In his essay on bloodletting, Haller covers some well-trodden ground, but he has a good section on the few perceptive physicians who as early as the sixteenth century began criticizing venesection. The chapter on "The Aging Materia Medica" concentrates upon three much-abused mineral remedies of the nineteenth century: mercurials, antimonials, and arsenicals. It is hard to say which is more intriguing—Haller's account of the horrendous dosages administered or the physicians' rationale for so doing. Under the descriptive title of "Transcendental Medicine" he includes a wide range of medical sects such as mesmerism, homeopathy, faith healing, and Christian Science. Haller shrewdly notes that Hahnemann, the founder of homeopathy, stressed that disease arose from spiritual and dynamic disturbances, and by so doing he influenced Christian Science and other forms of faith healing.

In the essay "Midwives in Britches" Haller notes that while midwives delivered the majority of babies in England and America neither country required

any formal training for midwives prior to the twentieth century. Most American physicians, facing fierce competition from within and without their ranks, had no intention of encouraging midwifery. By 1911 when the first good school for midwives was opened in the United States, the AMA was already gaining the upper hand in its fight to monopolize obstetrics. The last three essays cover medical education, the business and ethics of medicine, and the impact of evolution on medicine. Haller touches only briefly on a few of the major developments in education, but he does point out that many good physicians objected to improving medical education on the grounds that by eliminating students from the low-income class it would make the profession elitist. In the essay on business and ethics Haller shows clearly how the two were entwined in the medical profession. Since most of the early professional codes were concerned primarily with establishing fees, eliminating irregulars, and promoting professional etiquette, the line between economics and ethics becomes almost indistinguishable.

While in no sense presenting a complete picture of the period, Haller has written some perceptive essays and provided new insights into an intriguing period in American medicine. His work represents a fine synthesis of medical, social, and intellectual history.

JOHN DUFFY

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PETER TEMIN. *Taking Your Medicine: Drug Regulation in the United States*. Cambridge: Harvard University Press. 1980. Pp. vii, 274. \$18.50.

Taking Your Medicine explores the adoption and implications of three key legal controls in the American supply system for medicines: the premarketing requirement of evidence of "safety" (1938), the enforced division into prescription and nonprescription categories (formalized in 1951), and the requirement of evidence of "effectiveness" (1962). Complexities of defining and enforcing these requirements over the decades are examined "to clarify the goals of public policy toward medicinal drugs." A general history of pharmaceutical law and regulation, which the book's title might seem to imply, is not part of the task.

An uneasiness pervades the book about the federal government restricting the availability of drugs in these three ways, so that "consumers cannot choose drugs for themselves" and "doctors cannot choose drugs alone either." Thus, "the central question of this study is whether these paternalistic assumptions are justified and whether their current manifestations can be defended" (p. 17). Peter Temin has not been able to answer the question definitively on

historical grounds. That he has made a systematic and scholarly attempt, however, merits attention by anyone seriously interested in the history of medical care or in the continuing public policy issue.

The author concentrates on the period since passage of the Food, Drug, and Cosmetic Act of 1938. He relies heavily on ideal typology drawn from social science as an interpretive tool. Especially pervasive is the use of instrumental, customary, and command modes of human behavior to clarify the changing impact of regulations on interacting groups: consumers, health professionals, drug manufacturers, and government personnel (FDA).

More than half the text is devoted to chapters dealing with the regulatory legacy of the New Deal, the striking innovations in drug therapy between the 1930s and 1960s, the perplexing relationships between physicians and the marketing and prescribing of drugs, the expansion of drug regulation after World War II, and the influence of social changes on the drug-marketing process. The last fourth of the text administers a rather large dose of economic theory and model manipulation—aided by diagrams and an institutional typology of hierarchy, market, and community—to help “formulate views about current policy choices” and provide some “guidance for prediction.”

As an economic historian, Temin seems most original and penetrating when explaining such facets as the structure and process of drug marketing, the social impact of different stages of regulation, and the shifting locus of decision making about medicines—all part of that broader conflict between the political concepts of individual liberty and of consumer protection. He is understandably less successful in dealing with the therapeutic and pharmaceutical side, such as the ramifications of the cost or benefit equation that is so central to *Taking Your Medicine* (compare, for example, William Wardell and Louis Lasagna, *Regulation and Drug Development*). Moreover, it is hard to find the evidence for some of the historical facts and interpretations given. For example, was “the risk of overpaying for a drug” really a central motivation for the 1906 Food and Drugs Act? Is it cricket to imply that the Congress or the FDA believes all risks can be eliminated from taking medicines (pp. 1, 3)? Is there any evidence before the present century that “most people taking medicines were . . . reasonably healthy” (p. 4)? What evidence is there that Harvey W. Wiley’s campaign for food and drug legislation was more concerned with “petty fraud” than “with any danger to the well-being of the public” (p. 28)? How can it be said that “drug companies in the 1930s did not advertise to doctors” (p. 58)? Is there no difference between an innocuous and an inert drug (pp. 126, 137)? Who has read the 1906 act and concluded that FDA’s predecessor agency “was restricted to informing the consumer about the com-

position of drugs” (p. 198)? These minor lapses cumulate to a point where a reader without some background in the history of drug control is at a disadvantage.

Taking Your Medicine comes off best when viewed as a critique of one strand of drug regulation, one that entails a conflict of values full of promise for further decades of discussion. Traversing this thorny terrain, with Temin as a well-informed guide, proved to be a more rewarding experience for me than arriving at his destination of prescriptions for new kinds of command-dominated controls.

There is excellent documentation, a separate bibliography, and a five-page index.

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CLETUS E. DANIEL. *Bitter Harvest: A History of California Farmworkers, 1870–1941*. Ithaca: Cornell University Press. 1981. Pp. 348. \$19.50.

In this tightly structured, well-documented book, Cletus E. Daniel examines the explosive labor conflicts in California agriculture before World War II. Corroborating Carey McWilliams, he traces their origin to the peculiar character of the state’s large-scale, highly capitalized farming with its dependence on a mobile, compliant, low-paid work force. Daniel quotes growers—unable to reconcile their commercial enterprise with agrarian ideals—justifying labor practices that created a “captive peasantry” on the grounds that the workers they employed were “socially marginal” white migrants or members of racial minority groups.

California farmworkers under this caste system were ignored by the conservative American Federation of Labor but attracted organizers with revolutionary aims, first from the Industrial Workers of the World, later from the Communist party. Strikes called by radical unions exposed inhuman and degrading working conditions in the fields, thereby activating liberal reformers.

Daniel makes an important contribution to early twentieth-century labor history through his brilliant analysis of the ensuing three-way struggle between California growers, unionists, and government officials. He breaks new ground in documenting the ambivalence of the Progressives who were simultaneously prolabor and antiunion. Governor Hiram Johnson’s Commission on Immigration and Housing initiated labor camp inspections after a much publicized outbreak of violence in the hop fields near Wheatland in 1913 and then assisted in the nationwide prosecution of the IWW whose “job delegates” had organized the hop pickers’ revolt. Two decades later New Deal arbitrators in the

depression-era strikes substituted government-backed reforms for recognition of a farmworkers' union to counteract the organized strength of the growers.

While exposing the paternalism that undercut grass roots efforts to alter the balance of power, Daniel also demonstrates how internal problems in the radical unions limited their long-range effectiveness. The Communist-led Cannery and Agricultural Workers Industrial Union doubled wages in 1933 in one of the most successful series of farm strikes of all time, but, like the IWW, it had difficulty functioning as both a trade union and an instrument for revolutionary change. Organizers were criticized by party officials for neglecting ideology in favor of bread-and-butter issues. Yet while they tempered their message to match the pragmatic concerns of farmworkers, their radical affiliation resulted in their arrest under the state's criminal syndicalism law, which had originally been invoked against IWW leaders.

Farmworkers who had been deliberately left out of the new federal legislation covering industrial workers were at the mercy of the coalition of banks, utilities, railroads, and large growers known as the Associated Farmers, Incorporated. Daniel touches on the lackluster organizing by the CIO's Cannery, Agricultural, Packing and Allied Workers of America against this opposition in the late thirties.

The final chapter is a too cursory summary of these later events. Daniel ignores such details as Governor Olson's liberalized policy on relief payments or the Farm Security Administration's change of rules on labor organizing at the camps for Dust Bowl migrants. Both concerned the Associated Farmers whose excesses were exposed by the La Follette committee in 1939.

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CHARLES L. WOOD. *The Kansas Beef Industry*. Lawrence: Regents Press of Kansas. 1980. Pp. xiii, 352. \$22.50.

Charles L. Wood opens his solid monograph on the Kansas beef industry by observing that compared to the nineteenth-century range cattle industry, the historiography of the twentieth-century ranching industry is a glass of Perrier indeed. Moreover, contrasted to the nineteenth-century cattle industry, the twentieth-century cattle industry has become "more rationally organized and efficient in production, more representative of improved stock, and more concerned with industries associated with marketing and with the actions of the government" (p. xiii).

Wood then guides the reader slowly through the ebb and flow of the evolution of the Kansas cattle industry—twentieth-century style. The upgrading

of the range cattle (wherein the cattlemen in Kansas, as elsewhere, favored the Hereford breed above all others), the struggle for centralized marketing, contests over freight rates, the development of the Kansas Livestock Association, boomeranging production costs, vacillating government regulation and nonregulation—all form a part of Wood's narrative.

Missing from this account is the tale of the encroachment of competing agriculturists, the suitcase farmers, who surged over the land. As one Nebraska rancher once stated, "Everywhere I turned, those damn farmers were there—just like locusts." An examination of the twentieth-century rancher's view of his neighbor on the adjoining 640 acres might have produced attitudinal patterns remarkably similar to his nineteenth-century ancestor. Missing also is that answer as to why today a cattleman is a cattleman. (Dan Casement's explosions, as irreverent and outrageous as they are, would provide some insight). The cattleman's devotion to his "place" (a feeling sometimes suspended in time), his dedication to his ranch as a monument to a way of life—all merit investigation. The same human element that has recently surfaced with so much emotion in the current agriculture-energy contretemps. All of this Wood has squeezed from his story.

Perhaps the dehydration was intentional, for Wood wanted to escape the romance of the nineteenth-century setting to excavate the concrete stage of the twentieth century, specifically the rancher as businessman. The metamorphosis from the cattleman's disorganized business practices of the nineteenth century to the codified economic state of the twentieth century is, in retrospect, hardly surprising. In an industrial age, the cattleman, just as the neighborhood grocer and the small one-product manufacturer, had to alter his economic milieu or vanish from the scene. The cattle industry of the twentieth century has become, uncontestedly, big business, run by businessmen with commercial techniques. A testament to that recognition came recently from one northern plains cattleman, when he said, "if I had a son today, I would tell him to go down to that University and take accounting and law. These agricultural subjects we can teach him at home."

As those who have the good fortune to read Wood's masterful account of twentieth-century cattlemen will realize, he has made a contribution to the historiography of the cattle industry, and a major one—a chronicle, as far as Kansas is concerned, which will not need to be written again. Of course, it will be!

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RONALD L. FEINMAN. *Twilight of Progressivism: The Western Republican Senators and the New Deal*. (Johns

Hopkins University Studies in Historical and Political Science, Ninety-ninth Series, number 1.) Baltimore: Johns Hopkins University Press. 1981. Pp. xiv, 262. \$18.50.

The relationship of the Progressive movement to later American history is a subject that intrigues historians. Some years ago, in *An Encore for Reform*, Otis L. Graham, Jr., explored the attitude toward the New Deal of those old Progressives who had lived on into that era. Rather surprisingly, he found considerable hostility to many FDR measures. Now Ronald L. Feinman, in a more restricted study, analyzes the role played in the 1930s by the bloc of twelve Western Republican senators, derisively labeled "Sons of the Wild Jackass" by conservatives. All of the twelve singled out by Feinman—William Borah of Idaho, Arthur Capper of Kansas, James Couzens of Michigan, Bronson Cutting of New Mexico, Lynn Frazier of North Dakota, Hiram Johnson of California, Robert La Follette, Jr., of Wisconsin, Charles McNary of Oregon, Peter Norbeck of South Dakota, George Norris of Nebraska, Gerald Nye of North Dakota, and Henrik Shipstead of Minnesota—were highly individualistic and independent-minded, a fact that frequently limited their ability to exert political influence. They were united, however, in their disillusionment over the conservative policies of the Republican party.

When a third party proved impractical, the Progressive senators helped organize the National Progressive League for the Election of Roosevelt and Garner. But curiously, none of the bloc was willing to sacrifice his freedom of action to enter the Roosevelt cabinet. What the Progressive senators wanted were larger federal expenditures for public works and unemployment relief, plus FDR's backing in their own 1934 re-election campaigns. For a short time, Senator Borah indulged the hope that he might lead the Republicans toward a more Progressive stance. Instead, Borah and his fellow Progressives suddenly found themselves no longer needed by the Democrats after FDR's sweeping electoral success in 1936. The president's Supreme Court reorganization fight further disillusioned all except Norris and La Follette.

Legislation in behalf of labor and mounting federal expenditures alienated the more conservative of the original twelve Progressive senators, all of whom came from rural Western backgrounds. Nevertheless, as Feinman makes clear, it was foreign policy that brought about the final break. By 1938 Norris, grateful to Roosevelt for helping him achieve his dream of the Tennessee Valley Authority, remained the only one among the isolationist Progressives who supported Roosevelt's interventionist diplomacy. And Norris voted against military conscription. Their opposition to American entrance into World War II ultimately forced most of

the Progressives into political oblivion. "Twilight" accordingly seems an appropriate term for describing the status of the Progressives in the 1930s. Their honeymoon with Roosevelt and the New Deal had been short and troubled.

Feinman tells his story with commendable objectivity and no little sympathy. In a cynical age, it is worth remembering that the Progressive senators, more than most politicians, were able to maintain a notable sincerity and integrity in their public views. The *Congressional Record*, along with biographical data, furnishes the bulk of the sources. From among manuscript collections, the author quotes some particularly revealing letters in the Hiram Johnson papers.

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ROBERT DALLEK. *Franklin D. Roosevelt and American Foreign Policy, 1932–1945*. New York: Oxford University Press. 1979. Pp. xii, 657. \$19.95.

In this long-awaited volume, Robert Dallek sets out both to provide a comprehensive account of Franklin D. Roosevelt's foreign policy and to "wrestle anew with the many intriguing questions about that subject" (p. vii).

Dallek succeeds admirably in fulfilling his first objective. He gives an excellent recapitulation of FDR's long and involved foreign policy record. Relying on a chronological approach, Dallek achieves a nice balance between FDR's personal contributions and the nature of the problems with which the president dealt. The prose is polished, the coverage thorough, and the judgments clear. Dallek is especially good on the intricate international economic issues that FDR faced in 1933 and on his convoluted wartime policy toward China. On the latter, he demonstrates that Roosevelt, far from pursuing a romantic will-of-the-wisp, was in fact using China as a counterweight against both Britain and Russia as well as betting on China's emergence as a great power in the future.

The research is impressive. Dallek displays a thorough knowledge of the large body of secondary literature on the thirties and World War II, yet he also explores the more important primary sources. As a result, he goes beyond a mere summary of diplomatic monographs to give his own reading of events, usually confirming but occasionally challenging the conventional view.

He is much less successful, however, in fulfilling his goal of probing the mysteries of Roosevelt's approach to foreign policy. Dallek's greatest handicap here is that he sees everything just as Roosevelt did and thus lacks perspective. Yes, Roosevelt ap-

peased the isolationists in the thirties but only to outmaneuver them later. Yes, Roosevelt lied to the American people in 1940 and 1941 but only in their own best interest. And yes, he did give in to the Soviets on Poland at Yalta but only to ensure postwar cooperation. Even when Dallek is critical of FDR's moves, as in the Spanish Civil War or in the neglect of Jewish refugees, his judgments are mild and forgiving.

Dallek's fundamental argument is that Roosevelt's policies must be judged in the context of the times. FDR was not a free agent. He had to adjust to the prevailing isolationist mood of the 1930s; he could not confront the Axis powers directly in 1940 and 1941; he needed Russia more than Russia needed him during World War II. Throughout the book, Dallek uses the words "domestic pressures" and "international constraints" to demonstrate Roosevelt's limited freedom of action. "Roosevelt felt immobilized by international and domestic constraints" (p. 264); "pressures beyond his control pushed Roosevelt toward a confrontation with Japan" (p. 273); his postwar vision failed to materialize because "Russian expansion, Chinese strife, and colonial revolutions were beyond Roosevelt's power to prevent" (p. 537).

In stressing the limitations on Roosevelt, Dallek undermines his historical stature. A great leader rises above events to achieve his goals or at the very least manipulates them to his own advantage. Dallek is least convincing in dealing with Roosevelt as the prisoner of American public opinion. FDR's tactic, we are told, was to deceive the public, telling the people what they wanted to hear while actually doing the opposite. Thus he led the nation into war while proclaiming neutrality, and he presented the Yalta agreements as the embodiment of national self-determination while secretly conceding Soviet domination over Eastern Europe. On both occasions, Dallek defends FDR for pursuing a realistic course and justifies his devious public statements on the grounds that the American people did not know what was best for them. Nowhere does Dallek suggest that the role of a leader in a democracy is to inform and educate the public to meet its international responsibilities.

Dallek's defense of FDR as the accomplished pragmatist presents one further difficulty. Even the most gifted opportunist must strive for some ultimate goal, some enduring principle that justifies his acts of expediency. The most damning part of the book is the portrait of FDR as utterly amoral. Thus Dallek shows him apathetic to the plight of Jewish refugees, callous toward the fate of Poland, and absolutely vindictive toward the defeated followers of Hitler, telling Henry Morgenthau that it might be necessary "to castrate the German people" (p. 472). In his deliberate attempts to avoid the pitfalls of

Wilsonian idealism, Roosevelt went too far in the other direction, yet the author never discusses the ends that justified FDR's dubious means.

Dallek also fails to probe deeply into Roosevelt's unorthodox method of conducting foreign policy. He does tell us that Roosevelt believed in a divide-and-rule system whereby he deliberately pitted subordinates against each other, most notably Cordell Hull and Sumner Welles, so as to force them to come to him as the court of last resort. Dallek evidently approves of this system, since he never discusses the enormous cost it entailed in terms of personal bitterness, duplication of effort, and interagency feuding. Roosevelt may have found this device useful in retaining ultimate control over American foreign policy, but he left a legacy of bureaucratic rivalry between the White House staff and the State Department that has plagued the nation ever since.

Despite these flaws, Dallek's book is impressive. He is fair and objective in the best sense, presenting all the evidence so that his readers are free to arrive at their own conclusions. This is no lawyer's brief for FDR, no one-sided defense that rests on selective presentation of the evidence. It is, rather, a lucid and comprehensive account of Roosevelt's diplomacy. Historians will continue to dispute the meaning and wisdom of FDR's international behavior, but from now on they will do so on the basis of Dallek's splendid recapitulation of Roosevelt's foreign policy.

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PRZEMYSŁAW GRUDZIŃSKI. *Przyszłość Europy w koncepcjach Franklina D. Roosevelta (1933–1945)* [The Future of Europe in the Ideas of Franklin D. Roosevelt (1933–45)]. Wrocław: Ossolineum. 1980. Pp. 215. 55 zł.

The evidence for Franklin Roosevelt's long-range thinking on Europe, as Przemysław Grudziński makes clear, is fragmentary, and what there is of it, in common with much of the man's thought, is superficial, inconsistent, and not clearly spelled out. Grudziński, like many American interpreters before him, argues that Roosevelt compensated for his intellectual deficiencies with the talents of an extraordinarily political animal. Since he also reminds us of Roosevelt's ability to articulate ideal goals for a mass audience, we are left with another version of the two-Roosevelts biographical approach, something in the tradition of the lion and the fox. Grudziński tries, successfully, to balance both pragmatism and vision in the development of Roosevelt's plans for the future of Europe.

Given the character of the president, we should

not be surprised that Roosevelt changed his mind significantly at least twice in thinking about Europe and that he should have left American opinion and officials well behind him. Pessimistic about the vitality of the old order in Europe and fearful of the hostility of the new order, Roosevelt abandoned his faith of the late 1930s in collective security as the basis of European stability. (Europe is defined here as the continent from the Atlantic to Poland.) Although official American statements continued in a Wilsonian vein, Roosevelt arrived by 1942–43 at a vision of a world governed by a four-power directorate in which American moral and material might would be dominant. Then, overestimating the permanence of the changes in power relationships wrought by the war, Roosevelt thought increasingly in terms of an American-Soviet condominium in which a weak, disarmed, and restructured Europe existed harmlessly on the periphery of world affairs. It was within that framework that he tacitly conceded Soviet preponderance in East-Central Europe in 1943.

Building upon American revisionist historiography from the early 1970s, Grudziński believes that Roosevelt assumed that the United States—morally strong, its economy intact, and quite possibly armed with atomic weapons—would be from a global perspective the dominant partner. Meanwhile, policy makers in the State Department, official pronouncements, and congressional leaders continued to focus on international organization and an essentially Wilsonian program for the postwar world. Public opinion in the United States and Europe was even further removed from the president's growing commitment to a permanently interventionist foreign policy.

All of this of course illustrates a not uncommon division of purpose and execution in American government policy. It was also typical of Roosevelt (and, one might add, of the imperial presidency) to be secretive, distrustful of official channels and expertise, and reliant upon personal relationships. Perhaps a workable relationship with Stalin was possible. The death of its chief proponent made the question moot. Grudziński touches on the origins of the Cold War only incidentally, but his careful use of the major printed sources (unfortunately he did not consult any of the relevant manuscript collections directly) helps to define Roosevelt's part in shaping postwar Europe. Unable or unwilling to generate wide support for his personal vision, incompletely understanding European history and nationalism, Roosevelt left the United States to confront the European power vacuum of 1945 with choices that could only contribute to Cold War diplomacy.

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AKIRA IRIYE. *Power and Culture: The Japanese-American War, 1941–1945*. Cambridge: Harvard University Press. 1981. Pp. ix, 304. \$22.00.

Power and Culture takes a different approach to the Pacific war, seeking to move beyond its political and military dimensions to an analysis of the meaning Americans and Japanese gave to that conflict. As in previous books, Akira Iriye employs his mastery of a wide range of sources to construct an interesting account of the interplay between American and Japanese conceptions of the war and of the postwar world. He believes that international relations must be studied as "power-level interactions, cultural interchanges, and the relationship between these two sets of relations" (p. vii) and views the Pacific war as a "case study" (p. viii) of this approach.

Iriye explores the origins of the war between the United States and Japan, emphasizing the unsystematic, opportunistic nature of Japanese policy and the way in which both governments exaggerated their differences. But the heart of the book deals with the war itself and the attempts of the two nations to understand it. Japan's new order in Asia was an abstract, poorly developed concept, one that was half-heartedly pursued and quickly sacrificed to military necessities. In Southeast Asia the Japanese, despite all their pan-Asian rhetoric, found it easiest to replace European colonial rulers, and in China they were reluctant to give much authority to Wang Ching-wei's regime in Nanking. In China Japan confronted most dramatically the gap between its pretensions and the reality of its rule, and Iriye provides much fascinating information on Japan's futile efforts to mobilize China's masses and to prove that the Chinese people were eager to take their place in Japan's new Asian order. China presented dilemmas for American statesmen as well, for the confusion and uncertainties of the Chinese revolution made it difficult to predict what role China would play in postwar Asia.

As the war progressed, and Japan's military fortunes declined, Japan moderated its policies in China and Southeast Asia. Many prominent Japanese remembered the 1920s, when Japan was part of a cooperative, interdependent world order, and they gradually realized that military defeat, while it would end Japan's imperial dreams in Asia, would bring the reintegration of Japan into an American-inspired postwar international system. In the United States, too, State Department officials drew on the past to form an optimistic vision of the future, concluding that Japan, once purged of military and political extremists, would again become a partner of the West. Iriye examines in great detail the deliberations of State Department planners and argues that they developed a remarkably clear vision of postwar Japan. On both sides of the Pacific, then,

Iriye suggests that a gradual convergence of war aims occurred, as moderates in each nation realized that the bitter military struggle obscured shared assumptions and aspirations, and that the war was more a conflict of power than of ideals.

This intriguing insight leads Iriye to regret that the war did not end earlier and to berate Japanese leaders for their slowness in reformulating war aims and in directly approaching the United States. He is preoccupied with the might-have-beens of the Japanese-American war, with the possibilities of ending it in 1944 or May 1945, if only Japanese leaders had acted with more boldness and had not exaggerated the difficulties of persuading the military to accept the inevitable. As Iriye observes, an earlier conclusion of the war, while it would have spared much suffering and destruction, would not have altered the shape of postwar Asia, although it would have prevented the dropping of the atomic bomb and the sudden, brutal dawn of the nuclear age. But these speculations, while challenging, seem tentative, for Iriye does not explore in any depth the complex array of forces in Japan toward the close of the war or develop his thesis in a really compelling, systematic way. As a result, the reader is left with fresh insights but without a convincing reinterpretation of the conflict between the United States and Japan.

CHARLES E. NEU
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FREDERICK L. SHIELS. *Tokyo and Washington: Dilemmas of a Mature Alliance*. Lexington, Mass.: D. C. Heath. 1980. Pp. xiii, 202. \$20.50.

"Policy science theory" is the tag that best describes this book. The Japanese-American experience as allies has been dissected, fitted into an analytical frame, and employed to predict the future of the alliance. This is action-oriented scholarship, a book that has been explicitly designed to influence policy.

Portrayed here is an alliance that has proven marvelously complex. No other such relationship, it is argued, has involved such intimate ties of trade, military dependence, and mutual political support, while simultaneously subjected to such powerful countervailing forces as disparate cultural, religious, and psychological traditions. During three decades, moreover, the alliance has been battered by recurrent misunderstandings, crossed national purposes, and dramatic shifts in the fortunes of the two partners. Yet, despite the manifold strains, the alliance promises to remain during the 1980s a vital force in world politics. If Japan—now an economic superpower and inclined toward demonstrations of independence—seems ever more likely to seize the initiative in foreign policy matters, it nevertheless will continue to defer overall to American leadership. The pattern will continue to reflect the depen-

dent behavior that was cultivated during the American occupation of Japan.

These conclusions are less remarkable than the methodology that was employed to reach them. Frederick L. Shiels has constructed a two-tier analytical model for testing the vitality and directions of alliance politics, one that, as compared with those prepared by other political scientists, is uncommonly complex. He proposes that an alliance first be tested in the context of international politics. Here commitments between nations are subjected to dynamic forces, which constantly alter the perception of a government as to an alliance's worth. Shifts in ideological commitments, the transformation of national policies, the formation of competing alliances, or the accumulation of misunderstandings figure importantly among the forces that may erode once seemingly vital international relationships. Thus, predictions concerning alliance politics start with a systematic inquiry into how well a particular arrangement has withstood the inevitable strains of international politics. Then, as a second step, there should be a close examination of the foreign policy processes through which nations determine what they will do with respect to their alliances. To accomplish this Shiels would have us employ a method that synthesizes efforts to comprehend state behavior through analysis of what makes decision makers act as they do (an approach most often identified with scholars like Richard Synder or Glenn Paige and the theories of Graham Allison, Mortin Halperin, or John Steinbruner, who hold that decisions are the product of bureaucratic politics and organizational dynamics).

If this book illustrates once again the gulf between two disciplines that share an interest in foreign policy, diplomatic historians nevertheless ought to examine particularly the methodological sections of this book. Shiels's analytical method suggests approaches that can be adapted to historical research.

BURTON F. BEERS
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GREGG HERKEN. *The Winning Weapon: The Atomic Bomb in the Cold War, 1945–1950*. New York: Alfred A. Knopf. 1980. Pp. x, 425. \$15.00.

The Winning Weapon is a narrative examination of the atomic diplomacy of the United States in the years after the end of the Second World War. Gregg Herken tells his story by skillfully inserting his account of the changing details of American strategy into a more general outline of the Cold War. This outline encapsulates a by now conventionally moderate viewpoint between revisionism and orthodoxy. But Herken's ideas in his own area of interest add a significant dimension to our understanding. He persuasively argues that American diplomats arro-

gantly believed they would have a monopoly on the secret of the atom indefinitely. This belief made them rely almost exclusively on the bomb in diplomacy. It made them inflexible in dealing with the Soviets—inflexible in the sense of being unwilling to negotiate and inflexible in the sense of refusing to find more creative diplomatic strategies to handle genuine difficulties with the Russians. Finally, Herken says, the mistaken belief in the nuclear monopoly precipitated a frightening and continuing national crisis when the Russians exploded their first atom bomb in 1949.

Herken's book is intelligent and well written and, I think, will gain the deserved respect of similar books in its genre—John Gaddis's *The United States and the Origins of the Cold War*, Martin Sherwin's *A World Destroyed*, and Daniel Yergin's *Shattered Peace*. At the same time *The Winning Weapon* has the defects as well as the virtues of these books. In his attempt to circumvent the standard complaint made against "traditional" diplomatic history—that it sticks to summarizing the exchanges among diplomats and ignores the social context of international affairs—Herken enlivens his narratives with bits and pieces of human interest: stories, jokes, blunt quotes, and so on. These devices achieve a certain success in making diplomacy come alive. But they give the reader a false sense that the "new" diplomatic history transcends the limitations of its "traditional" predecessor. It is far too strong to say that introducing human interest into diplomatic history is the equivalent of intellectual junk food. But it is true that such human interest is no substitute for the social analysis of foreign relations that William Appleman Williams was briefly able to champion successfully. The tolerant moderation in diplomatic history that has emerged from the orthodox-revisionist dispute—and Herken's book is an outstanding example of its success and its value—has nonetheless neglected a major truth of the revisionist analysis. This truth that Williams tried to teach us is that the examination of our diplomatic past must concern itself with the culture of international politics and the way in which foreign affairs reflect the essentials of the domestic order. *Magna est veritas et prevalebit*.

BRUCE KUKLICK
University of Pennsylvania

MICHAEL MANDELBAUM. *The Nuclear Question: The United States and Nuclear Weapons, 1946–1976*. Cambridge: Cambridge University Press. 1979. Pp. x, 277. \$14.95.

In *The Nuclear Question*, Harvard political scientist Michael Mandelbaum adapts the nineteenth-century wisdom of Clausewitz to a study of American nuclear weapons policy from Hiroshima to the

Carter presidency. Mandelbaum's analysis focuses on how that policy has been a response to the three questions of strategy that concerned Clausewitz: What are the political purposes to which a nation's military force will be put? How is that force to be deployed to serve those purposes? And how will the force be used once hostilities have begun?

The author argues that, after nearly twenty years and many idealistic but misguided efforts either to outlaw or to ignore the bomb, American statesmen finally came to accept the truth of what one particularly prescient civilian strategist—Bernard Brodie—recognized in 1946: "Thus far the chief purpose of our military establishment has been to win wars. From now on its chief purpose must be to avert them. It can have almost no other useful purpose." From this simple axiom proceeded first, deterrence, which became the centerpiece of American nuclear strategy; and later, when both sides had hydrogen bombs in quantity, the strategy of mutually assured destruction—whose ironic acronym defines the current situation of nuclear stalemate (Clausewitz called it "armed wariness") between the United States and Russia. Mandelbaum claims that both the theory and the fact of stable deterrence, which is at the heart of MAD, were the accomplishment of the Kennedy administration. Kennedy and the New Frontiersmen settled upon mutually assured destruction as the least abhorrent choice after a brief but unsuccessful flirtation with the alternatives to MAD: counterforce strategy, or the "no-cities" doctrine; passive (or civil) defense; and active defense against enemy missiles. Each alternative was rejected in turn by our allies, Congress, or ourselves. The acceptance of mutually assured destruction—at least until recently—by the United States and Russia represents, Mandelbaum concludes, a significant departure from the traditional answer that nations have given his third question, because of nuclear weapons. Thus the challenge to statecraft and to military science posed by the bomb is how *not* to use the weapons one possesses, since "absolute war" today would destroy those political objectives that past wars sought to obtain.

The Nuclear Question does not pretend to be a traditional diplomatic history, and, indeed, it contains little that is new concerning the period or its personalities. It is, instead, a grand treatise on the bomb in the tradition of Raymond Aron or Clausewitz's own work. Just because of this omnibus approach, the book occasionally seems to wander far afield from its topic—the warring of the medieval Italian city states is used to set the stage for the nuclear diplomacy of the Baruch plan, for example. In addition, some subjects of importance to the book's thesis—the evolution of deterrence and the development of the Sino-Soviet rift—are only dealt with in summary, or even cursory, fashion. Finally, the act of applying the early nineteenth century to

the last half of the twentieth seems sometimes strained. Clausewitz's notion of "friction"—roughly equivalent to Tolstoy's "fog of war"—is used by Mandelbaum to describe bureaucratic politics and congressional opposition as well as the effect of unforeseen events—a definition so broad that even Clausewitz might not recognize it. In the author's defense, a second volume of the trilogy Mandelbaum plans on the bomb has recently been published that seeks to set these weapons in historical perspective with prenuclear strategy and diplomacy.

Its flaws notwithstanding, Mandelbaum's ambitious study is an important and highly original study of how we came to be where we are in what used to be called "the delicate balance of terror." The overarching question that Mandelbaum (and presumably Clausewitz) asks—"What are these weapons for?"—remains the most important one today when rearmament, technology, and new theories of limited and "winnable" nuclear war have conspired to reopen the questions whose answers we had grown accustomed to, if not comfortable with.

GREGG HERKEN
Yale University

E. BRUCE GEELHOED. *Charles E. Wilson and Controversy at the Pentagon, 1953 to 1957*. Detroit: Wayne State University Press. 1979. Pp. 216. \$14.95.

To date we have had fifteen secretaries of defense; Charles E. Wilson was the fifth and had the second longest tenure. He was Eisenhower's first choice not because of any strategic insight that Wilson had displayed but because of his business background. Ike wanted a business manager at the Pentagon to keep the lid on the budget, and Ike himself would be the strategist. This is the way it worked out during Wilson's incumbency from January 1953 to October 1957.

Although Ike tolerated Wilson, he found the secretary, with his penchant for details, a cross to bear. Wilson never had the rapport with Ike that Dulles or Humphrey at Treasury achieved. Wilson's relationships with the military except for Admiral Arthur W. Radford, chairman of the Joint Chiefs of Staff, were stormy, especially with the army chiefs who considered that service shortchanged because of Eisenhower's technologically heavy strategy, the "New Look." General Matthew B. Ridgway, the army chief appointed in 1953, served only one term and shortly after leaving office was writing critically of Wilson. General Maxwell Taylor, Ridgway's successor, had his turn in 1959 with *The Uncertain Trumpet*, long after Wilson had left office.

E. Bruce Geelhoed tells the entire story in his book, from the furor over Wilson's confirmation to his departure about the time of Sputnik. The book

is thoroughly researched and clearly written. Reading through it one is struck again with the severe case of "foot-in-mouth" disease that Wilson suffered. In his own way he may have been the most colorful of the defense secretaries. It is clear, however, in the reading that whatever his managerial abilities he was an amateur strategist—a type that should no longer be appointed to that office.

The author correctly points out that Wilson's long tenure provided the stability that the office needed at that point. He also is on target in emphasizing Wilson's successful carrying out of Ike's mandate on staying within what the president perceived to be the limit on resources available for defense. Geelhoed describes Wilson's cabinet service accurately as "durable, pragmatic, and competent."

DOUGLAS KINNARD
University of Vermont

DOUGLAS KINNARD. *The Secretary of Defense*. Lexington: University Press of Kentucky. 1980. Pp. 252. \$19.50.

Although less than four decades old, the position of secretary of defense has evolved to a point at which it rivals the secretary of state in importance. This evolution, as seen through the tenure of four key secretaries and one president, is the subject of Douglas Kinnard's incisive study.

James Forrestal was the first of these officials. While believing that the postwar Soviet threat demanded a revitalized American military presence, Forrestal found himself confronted by President Truman's domestic-based fiat that the defense budget must be severely constrained. Hampered by inadequate authority and near insubordinate service secretaries, he faced the task of mollifying intense and conflicting military claims in a period of absolute budgetary ceilings. In the end, the conflicts destroyed him, yet his efforts established the foundation for the Department of Defense.

Kinnard, author of an earlier study on Eisenhower and defense politics, sees Ike more in control of policy than any other president before or since. Eisenhower served, in effect, as his own secretary of defense. Using his secretaries as mere business managers, he "dominated and frequently manipulated a powerful set of political and military appointees" (p. 71).

The Vietnam War witnessed two important secretaries, one who helped the United States into the war and the other who helped it out. Robert McNamara left a tremendous managerial impact on the Pentagon. But he failed on the issue of Vietnam, Kinnard argues, because too little civilian communication existed with the military and too little civilian control was exercised over it. The important questions were not asked, nor their answers sought.

By the time Melvin Laird became secretary, it was clear to him that America would no longer support the war. Thus, he skillfully pushed for Vietnamization as a way of responding to domestic pressure to withdraw while still leaving South Vietnam with a partial hope of survival.

Kinnard's study closes with James Schlesinger, who effectively concentrated his energy on rethinking strategic doctrine in response to growing Soviet power and in reversing the downward flow in defense spending. He was, in the author's opinion, a man "who came along at the right time" (p. 189).

Kinnard is a retired general with a Ph.D. in political science. Though his opinions will not find universal acceptance, he is persuasive, supporting his study with an exceptionally clear and disciplined prose mercifully free of social science jargon. His research, while broad, does contain a few gaps, as when he fails to explain why he interviewed Stuart Symington for the chapters on Eisenhower and McNamara, but not on Forrestal. Since many considered Symington to be a major source of Forrestal's problems, such omission is noticeable.

Kinnard has not meant this as a definitive history or theoretical study of the secretary of defense. Instead, it is intended to provide insights into that office, and he succeeds admirably. His work will be a stimulating primer on the secretary of defense for years to come.

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RICHARD J. KENT, JR. *Safe, Separated, and Soaring: A History of Federal Civil Aviation Policy, 1961-1972*. Washington: Government Printing Office. 1980. Pp. vi, 422. \$8.50.

Despite the pervasive impact of bureaucracies upon all levels of modern society, capable studies in administrative history remain all too few. This perceptive and carefully researched monograph by Richard J. Kent, Jr., part of a multivolume series dealing with the development of federal civil aviation policy since 1926, is therefore a welcome addition to a relatively neglected area of scholarship. The agency with which it is centrally concerned, the Federal Aviation Administration (FAA), was created in 1958 to unify previously fragmented regulatory functions affecting American aviation and, in Kent's words, to keep the air traffic under its jurisdiction "safe, separated, and soaring."

Successful performance of this mandate demanded effective mediation among a number of vocal and well-organized constituencies whose needs and wishes were frequently in conflict, for example, commercial airlines whose interests were represented

by the Air Transport Association (ATA), airline pilots represented by the Airline Pilots Association (ALPA), airport operators represented by the Airport Operators Council International (AOCI), and owners of privately operated planes represented by the Aircraft Owners and Pilots Association (AOPA). Functioning under the executive branch of the federal government yet dependent upon Congress for appropriations and legislative support, the FAA was also vulnerable to all sorts of political pressures, while the need to keep the nation's aerial defense capabilities unimpaired required effective liaison with military leaders. Finally, tensions within the FAA itself—such as those between top agency officials and the increasingly restive and militant air traffic controllers, who ultimately formed their own bargaining agent, the highly controversial Professional Air Traffic Controller Organization (PATCO), made fulfillment of the FAA's mission all the more difficult.

It is a major part of the merit of Kent's study that he shows clearly, in an effectively organized and well-written manner, how a succession of persons who held the demanding position of Federal Aviation Administrator—Najeeb Halaby (appointed by John F. Kennedy), William F. McKee (appointed by Lyndon B. Johnson), and John H. Shaffer (appointed by Richard M. Nixon)—wrestled within these conflicting conditions to promote the efficiency and safety of civil aviation in a period of political turmoil and incessant technological change. The interest of the book for nonspecialized readers is also enhanced by the fact that some of the most explosive issues of recent times impinge directly upon the topics with which it deals: the impact of the Vietnam War upon domestic institutions, the increasing significance of environmental concerns, and the rise of aerial hijacking and other forms of political terrorism, to name but a few.

Kent's sensitivity to the broader implications of these developments helps clarify, for example, how Lyndon Johnson's need to finance ever-deepening Asian military involvements deprived the FAA of funds that were badly needed to meet the demands imposed by the jet age and how the supersonic jet transport (SST) became a symbol of environmental degradation to an aroused public and its congressional representatives in the late 1960s and early 1970s. All in all, therefore, this is an informative and valuable study that needs to be emulated by other works dealing with the history of administrative agencies—if we are to improve our understanding of forces and institutions that affect us all.

W. DAVID LEWIS
Auburn University

LELAND R. JOHNSON. *Engineers on the Twin Rivers: A History of the Nashville District, Corps of Engineers*,

United States Army. Washington: Government Printing Office. 1978. Pp. vii, 330.

JAMES H. KITCHENS III. *A History of the Huntsville Division, U.S. Army Corps of Engineers, 1967-1976*. Washington: Government Printing Office. 1978. Pp. xvii, 180.

JOHN W. LARSON. *Those Army Engineers: A History of the Chicago District, U.S. Army Corps of Engineers*. Washington: Government Printing Office. 1980. Pp. xi, 307.

RAYMOND H. MERRITT. *Creativity, Conflict, and Controversy: A History of the St. Paul District, U.S. Army Corps of Engineers*. Washington: Government Printing Office. 1980. Pp. 461. \$15.00.

GARY B. MILLS. *Of Men and Rivers: The Story of the Vicksburg District*. Washington: Government Printing Office. 1978. Pp. xiii, 244. \$6.00.

Four of these books, those by Leland R. Johnson, John W. Larson, Raymond H. Merritt, and Gary B. Mills, are similar in purpose and format. Each surveys the history of a particular geographical unit within the U.S. Army Corps of Engineers. Each is written under contract by a competent professional historian and published in an attractive binding with numerous maps and illustrations and each traces the general evolution of corps functions. For some ninety years after its birth during the Revolutionary War, the corps consisted of a number of officers, at first mostly foreign, then West Point graduates, to whom Congress assigned miscellaneous tasks of exploration and survey. During this heroic age the engineers frequently proposed the dredging of harbors and rivers and the building of roads and canals. But constitutional scruples inhibited Congress from taking much action. The Civil War, in which engineer officers played a prominent role, was a turning point in corps history. The Northern victory loosened congressional purse strings for ever more generous appropriations for local projects, most of them to benefit navigation. The next great expansion of corps activities came after the great Mississippi flood of 1927, which prompted Congress to begin appropriating large sums for flood control. With these new responsibilities the corps grew rapidly, but except for a cadre of professional army officers the personnel became predominantly civilian. Since 1970 the functions and organization of the engineers have been significantly altered to develop recreational facilities and to study the environmental impact of its projects.

Although these four books trace a common pattern of development, each deals with problems peculiar to a specific area of the country. Johnson's

study of the Nashville district tells of the corps' efforts, first to improve navigation on the treacherous twin rivers, the Cumberland and the Tennessee, and then to provide flood control and hydroelectric power through multipurpose dams. The completion of Wilson Dam at Muscle Shoals in 1927 inspired the development of TVA and other projects. Larson's history of the Chicago district traces the deepening of the Chicago harbors and a score of other ports on the Great Lakes. It also explains the somewhat confused history of the Illinois Waterway linking Lake Michigan with the Mississippi River. Merritt's history of the St. Paul district is the longest and most scholarly of these studies. It is particularly instructive in showing how the somewhat unwilling engineers transformed the Mississippi from St. Louis to Minneapolis from a free-flowing river into a canal, "an engineered stairway with twenty-six locks and dams" (p. 205). Mills's history of the Vicksburg district tells how the corps improved navigation on a vital section of the lower Mississippi, together with important tributaries such as the Yazoo, the Red, and the Arkansas. Later Vicksburg became a leader in developing new flood control schemes involving levees, diversion channels, bypasses, floodways, multipurpose dams, and reservoirs.

James H. Kitchens's, *History of the Huntsville Division* differs from these other books because it deals with a unique unit, one without geographical boundaries, created in 1967 to build a ballistic missile defense system for the country. After local opposition stopped development of a site near Boston, the corps concentrated on sparsely populated locations in North Dakota and Montana. In making plans, negotiating contracts, and overseeing construction of these highly complex systems, Huntsville performed an extraordinary task. But the achievement was ephemeral. The partly constructed Montana project was abandoned when the SALT I treaties were signed in 1972. The installation at Grand Forks, North Dakota, permitted under the treaties, was completed in 1975, but Congress inactivated it less than a year later. Huntsville was given a mixed bag of new assignments, ranging from procuring equipment for the modernized postal service to supervising construction projects for Saudi Arabia.

Army Corps of Engineers personnel should find much to stir their pride in these officially sponsored histories. The general public—particularly local history buffs—will find them readable and especially interesting because of their old prints and photographs. Historians will derive much useful information from them but will regret that Mills has no footnotes and Kitchens no index. Except for Merritt's book on the St. Paul district, the books do not contain much critical analysis. Certainly the corps' achievements deserve praise. In a country where

there is a foolish prejudice against planning Congress has assigned to the engineers responsibility for anticipating future needs in more and more different categories: navigation, flood control, water supply, energy, and—most recently—recreation and conservation of resources. In its construction projects the corps has been responsible for spending enormous sums of public money and its record is refreshingly clean of charges of corruption. But the corps has been conditioned by training and experience to seek structural solutions. Hence its preference for straightening and constricting meandering rivers, dredging ever deeper channels, and bottling up wild streams behind high dams. Environmentalists deplore the corps' failure to consider alternatives that would cooperate with nature rather than interfere with it. The authors do a much better job of chronicling the engineers' achievements than in looking for possible flaws.

NELSON M. BLAKE
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EUGENE VICTOR WOLFENSTEIN. *The Victims of Democracy: Malcolm X and the Black Revolution*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xi, 422. \$16.95.

This is a fascinating and formidable book. Using Malcolm X's *Autobiography* (as reproduced by Alex Haley after long sessions with Malcolm), and a number of his speeches, Eugene Victor Wolfenstein examines the changing consciousness of that remarkable black youth, hustler, prisoner, revolutionary—through a Marxian prism of socioeconomic history, but also with a whole laboratory of Freudian devices.

His method is to start with specific events in Malcolm's life: the childhood memory of his father, a Georgia-born black militant (a Garveyite) being killed by white racists near his home in Lansing, Michigan; Malcolm's retreat into the life of a juvenile delinquent and criminal; his imprisonment for six years and his conversion to black nationalism and Islam; his release and growth as a leader in the Muslim movement; his break with Elijah Muhammed, his trip to Mecca, and his formation of the Organization of Afro-American Unity just before assassins gunned him down on a speaker's platform in Harlem in 1965.

Each of these events is studied against an admirably concise race-class history of its time and is also probed with the tools of Freudian psychoanalysis: projection and introjection; oedipal jealousy of the father's sexual relations with the mother; love-hate feelings toward both parents; and (blending Freud and Weber) charismatic group emotion as a falsified form of consciousness (though potentially "progressive" in certain situations).

I find Wolfenstein's reconstruction of the major events in Malcolm's life powerfully alive and his extension of Marx to the interpenetrating issues of race and class both intricate and clear-headed. As for his elaborate Freudian explanations, I must say (at the risk of having my own relationship with my mother carefully scrutinized) that, while I find them ingeniously constructed and provocative even at their most exasperating, they keep reminding me of a brilliant inventor developing a bizarre combination of pulleys and gears to open a door that has a doorknob that needs only to be seen, grasped, turned, and pulled.

In emergencies, there is a practical difference, and Wolfenstein does believe (following Marx's *Theses on Feuerbach*) that theoretical issues can only be resolved in practice. Indeed, it seems to me that in explaining Malcolm's complex feelings for his father and mother as oedipal or his attraction for a white woman as "epidermal fetishism" (here combining Marx and Freud), Wolfenstein is illustrating false consciousness by his own definition: a diversion from common sense reading of experience. I was happy to escape from this into his compelling narrative, his thoughtful historical sociology, and the feeling of a deep intelligence joined to a commitment beyond mere scholarship.

He is asking profoundly important questions: how does consciousness (direct, undistorted learning from experience) get "falsified," so that people have difficulty figuring out how to solve their problems. And how do people free themselves from this false consciousness, and indeed from those real conditions that distort their lives and their thoughts?

Wolfenstein approaches these questions through Malcolm X, asking: how did racism falsify Malcolm's consciousness (as child, as subordinate, as hustler), and how did he set out to be free (partially, as a Muslim, more fully, as a revolutionary)? Malcolm's self-education is suggestive. Provoked by spiritual and physical imprisonment, encountering other "prisoners" who had gone through similar changes, he began to reject the spectacles, the theories, given to him with paternal generosity by the "democracy" in which he lived. He began to think his own thoughts, construct his own logic, his own rules, his own rationality, and he turned to organized collective action to apply that newly found rationality. "What is logical to the oppressor is not logical to the oppressed," he told Harvard students the year before his death.

The suggestion is valid not just for blacks but for all of us trying to resist the distortion of our lives and thoughts by social structures of violence and injustice.

HOWARD ZINN
Boston University

GARY M. FINK. *Prelude to the Presidency: The Political Character and Legislative Leadership Style of Governor Jimmy Carter*. (Contributions in Political Science, number 40.) Westport, Conn.: Greenwood Press. 1980. Pp. xxvi, 225. \$22.95.

Normally, several years pass before a president's papers are available to researchers, and, in cases of purportedly sensitive materials, the years may stretch into decades. President Jimmy Carter's presidential papers may be available much sooner than usual, and it will be fortunate for researchers if this proves to be true. The reason for this optimism is that Carter gave his gubernatorial papers to the Georgia Department of Archives and History without restrictions within a few days after he left the governor's office. The papers were almost immediately made available for historical research. The early appearance of this volume—based almost exclusively on these papers—is a direct result of Carter's decisions.

Carter's major contribution as governor of Georgia was his successful efforts to reorganize the state government, and this volume limits itself to that subject. After detailing Carter's break and fight with Lester Maddox on reorganization, this volume gives a careful account of Carter's efforts to "sell" reorganization to the people, the bureaucracy, and the legislature. The final chapters tell how Carter confronted the Georgia legislature—and won. The only shortcoming this reviewer can see in this well-researched, well-organized, and well-written little volume is that it reveals the cracks in the armor of Carter's enemies but never suggests a Carter flaw. Perhaps this work's major significance lies in the introduction in which the author speculates about the temperamental pragmatism of Carter.

The title of this volume is significant. If Carter had never been elected president of the United States, this volume would probably not have been written—indeed, it probably would not have been justified. But since Carter did become president, it might be instructive to view in detail his legislative leadership style at the state level. When historians research the Carter presidential papers, they may be able better to understand Carter's relationship to the United States Congress because of the availability of this work.

MONROE BILLINGTON
New Mexico State University

PETER D. MCCLELLAND and ALAN L. MAGDOVITZ. *Crisis in the Making: The Political Economy of New York State Since 1945*. (Studies in Economic History and Policy; The United States in the Twentieth Century.) New York: Cambridge University Press. 1981. Pp. xvi, 522. \$24.95.

"To ring a firebell in the night" is the avowed intent of Peter D. McClelland and Alan L. Magdovitz in this angry but scholarly study of the economic decline and financial woes of New York, state and city. Their anger makes for spirited writing and scathing judgments, but they sustain their case with a thorough analysis of the fiscal, legal, and economic issues. Legislators, capital correspondents, bankers, and academics will be using and citing this book, which does a fine job of clarifying a complex subject. The last third of the book is a compendium of statistical tables and appendixes as well as a good source for additional reading.

The authors devote the first two chapters to the economic retardation that began some fifty years ago, became noticeable after World War II, and took on alarming proportions in the past decade. They examine the usual catalogue of factors from high taxes to hostile business climate. They document the exceptional rate of outmigration of firms especially to neighboring states and, in the 1970s, to the Sunbelt.

Next comes an assessment of the policies of governors who enjoyed unusual powers in the Empire State. Thomas E. Dewey and Averell Harriman proved cautious in their tax and spending policies. Then the "hurricane" (author's term) struck. Nelson Rockefeller, an activist, problem solver, and big spender, littered the landscape with dormitories, mental hospitals, and office buildings. Standing in the way of his grandiose plans was the constitutional requirement that voters had to approve bond issues. Rockefeller evaded this requirement by establishing public authorities after the example of Robert Moses. Soon more than \$12 billion of "moral obligation" bonds were floated, almost four times the regular debt. The authors assail legislators for failing to scrutinize the gubernatorial budgets, and they censure judges for silent acquiescence.

The chapter on public authorities reviews the origins of these agencies, their abuse, and their threat to democratic government. Legislators, newspapermen, bankers, and the public receive blame for allowing Rockefeller and bureaucrats to ride roughshod over the constitution and sound fiscal procedures. The authors also indict the political leaders of New York City who caved in to the demands of civil servants and indulged in bizarre accounting practices. During the years when the city was losing its economic base, its mayors were granting more services such as open admissions to City University and extravagant pension benefits. Bankruptcy was narrowly averted in 1975, but the authors predict financial difficulties and more losses in manufacturing jobs.

This analysis of New York's trauma is clearly one-sided because all those billions of dollars did yield some constructive results: humane social programs

and ample educational opportunities, not to mention monumental quarters for state employees. Those aspects are sympathetically described in Robert H. Connery and Gerald Benjamin, *Rockefeller of New York* (1979). *Crisis in the Making* documents the final sentence in *Rockefeller of New York* that the governor tried "to do too much too fast" (p. 439).

DAVID M. ELLIS
Hamilton College

CANADA

JENNIFER S. H. BROWN. *Strangers in Blood: Fur Trade Company Families in Indian Country*. Vancouver: University of British Columbia Press. 1980. Pp. xxiii, 255. \$24.00.

Although the fur trade of the Hudson Bay watershed was opened by traders from two different business systems, one based in the St. Lawrence valley and the other in England, economics dictated that in both cases the trade would be conducted from small outposts staffed exclusively by men. For both sexual and policy reasons, on both white and Indian sides, these men often developed sexual liaisons with Indian women. The significance and consequences of these relationships are the main subject of Jennifer S. H. Brown's book. In earlier years, she argues, the two systems encouraged distinct patterns of "marriage according to the custom of the country." The Hudson's Bay Company's policy of establishing relatively more permanent posts, manned by poorly connected recruits from England, and the remoteness of these posts from the men's homes tended to lead to relatively more stable relationships and, quite often, to future roles in the company for the children of such marriages. By contrast, the North West Company drew its personnel from the relatively more accessible St. Lawrence Valley, although ultimately and increasingly its leadership consisted of clannish Scots. Its servants tended to maintain ties with and to return more often to the metropolitan society from which they came, and accordingly they tended to form temporary liaisons in fur-trade country.

In the early nineteenth century, as the fur trade and the European population in the northwest expanded rapidly, the number of country marriages (now often to mixed-blood daughters of earlier such marriages) and of children from them rose apace. When competitive pressures then led to a merger and consolidation of the companies, one result was that these children reached maturity at a time of sharply diminished opportunity for company em-

ployment. At highest levels of the company, the North West Company tradition of country marriages was now preferred. Moreover, the values of European civilization began to be felt more immediately in the northwest as, for example, European clergy and teachers and even some white women came west. All this sharpened racial, social, and economic distinctions in the northwest: changes in family structure and values were, therefore, among the pressures driving the mixed-blood population of the northwest to become increasingly conscious of itself as a distinct people.

When thus compressed this story has some familiar elements, but Brown brings to it the perspective of her training as an anthropologist, which makes her especially sensitive to nuances of status, kinship, culture, and intergenerational relationships, as well as to the patronage implications of these. This viewpoint means that *Strangers in Blood* should interest an audience wider than regional and fur-trade specialists only. Although full evidence such as a demographer would wish to have does not survive, and although the documents inevitably focus on the officer class of the fur trade, which left written records, Brown uses her evidence imaginatively to show the changing nature and role of country marriage and the changing opportunity structure for the children of these marriages. In an otherwise well-produced volume, the role of the twelve pages of illustrations provided is unclear; not only are they mostly from a later period, but also they are nowhere referred to in the text.

DOUGLAS MCCALLA
Trent University

JOY E. ESBEREY. *Knight of the Holy Spirit: A Study of William Lyon Mackenzie King*. Buffalo, N.Y.: University of Toronto Press. 1980. Pp. viii, 245. \$20.00.

William Lyon Mackenzie King, the dominant figure of Canadian politics from 1919 to 1948, was a peculiar character. He was, after all, a spiritualist who participated in séances, a man who reconstructed "ruins" at his country estate, a bachelor who kept in his study a large, constantly illuminated picture of his mother, and the owner of an Irish terrier that, King believed, was in close communication with his deceased mother. Since King's death in 1950 the public has been titillated with revelations from his voluminous diary, while scholars have struggled to interpret the relationship between King's public and private lives. C. P. Stacey's explanation is that King kept his "emotionally and frequently irrational private life separate from his rational public world." Joy E. Esbrey's *Knight of the Holy Spirit*, in contrast, claims that King's personality both influenced and was shaped by political incidents and activities.

Esberey's attempt to integrate the public and private King is organized into three generally chronological segments. The first examines the origins of King's so-called neurosis using Erik Erikson's concept of the life cycle and the insights of Freudian psychology. The author argues that King's failure to develop a mature male adult identity left him plagued by a tension-ridden relationship with his family, an excessive appetite for the plaudits of "important others" and an inability to establish a mature love relationship with a woman. The second part discusses the defense mechanisms King developed for "shoring up" the "insecure foundations": a work identity; a great capacity for self-delusion (in regard to his mother, for example); a substitution of verbal for physical intimacy; and a sense of being a chosen person, one directed by God rather than by himself. The final section analyzes selected examples of the interaction of King's personality and his politics during his long years of political power. Esberey maintains that in the Liberal party King found a fine family substitute; that King's concept of being divinely guided enabled him to maintain some idealism and consistency throughout his political career; and that his dislike of acrimony, his hunger for approval, and his self-image as a peacemaker strongly influenced his approach to labor relations, domestic politics, and foreign affairs. *Knight of the Holy Spirit* demonstrates that King's neurosis was not incapacitating. Rather, his coping mechanisms were well suited to a political career, for they gave him ambitious drive coupled with a sense of vision and integrity.

Esberey's work, the first full-blown psychoanalytic study of King, is largely successful in combining King's private and public worlds and in demonstrating the impact of personality in politics. Whatever doubts a layman might have of the accuracy of Freudian psychoanalysis, particularly of an individual who is not on the couch, *Knight of the Holy Spirit* has the ring of validity. Its conceptual framework is plausible, its organization logical, its research good, its writing acceptable, and its analysis perceptive.

Of the numerous valuable insights provided by Esberey, this reviewer found particularly interesting her assessment of King's handling of labor disputes. This was characterized, she maintains, not by opportunistic compromise but rather by a peculiar form of consensus: "The settlement was the plan that he deemed wisest; it was not a compromise worked out either by the disputants or to please the disputants" (p. 198). This analysis helps to explain both why King frequently turned a deaf ear to the concerns of the antagonists and how he sustained a sense of mission and leadership while ostensibly merely assisting two sides to talk to one another.

Readers will not discover a new King in Esberey's work, but their understanding of the man will be

enhanced. It enables one to transcend superficial amusement or disgust with King's foibles. *Knight of the Holy Spirit* is, in short, a welcome contribution to Canadian historiography.

WILLIAM M. BAKER

University of Lethbridge

LATIN AMERICA

MAURICE HALPERIN. *The Taming of Fidel Castro*. Berkeley and Los Angeles: University of California Press. 1981. Pp. x, 345. \$18.95.

It is the fate of the Cuban Revolution to have attracted the damaged but persistent utopian aspirations of the age. Auschwitz, Hiroshima, and the Gulag consumed as many hopes as souls, maiming the very notion of man-the-reasoner and thereby the larger search for a just state and a wise people.

Little noted outside the Caribbean before 1959, Cuba soon became the hope first of liberals and subsequently of socialists. The most recent Western society consciously to endeavor to re-create itself in idealistic terms, Cuba for a time served as the desperately needed proof that democracy could flourish in a Latin society and later that democratic socialism could emerge in a nonindustrial order. As evaluations of the revolution written in the 1960s described the felt betrayal of the liberals, so more recent volumes attest to a similar feeling among many socialists. As Castro once failed Theodore Draper, so he now fails Maurice Halperin.

This latest assessment of the Cuban Revolution is the work of a refugee from McCarthyism who, toward the end of an odyssey that took him to academic or consulting positions in Mexico, Brazil, and the Soviet Union, spent the years from 1962 to 1968 as a university lecturer and government economic adviser in Havana. The book, although it ranges more broadly on occasion, is essentially an examination of the speeches of Fidel Castro from 1964 to 1968. The author's interpretation of these speeches serves to support his contention that by the late 1960s Castro was "running one of the most tightly controlled, militarized and personalized dictatorships in the Communist world" (p. 313), had made a shambles of the Cuban economy, and was closely aligning Cuba with the foreign policy of the Soviet Union. Halperin is thoroughly cynical concerning the motives of the Cuban leader and delights in exposing inconsistencies and distortions in his public statements.

Despite the extremely narrow perspective and dependence on speculation for most of its principal

conclusions, the book is clearly the result of a close reading of the Cuban press by an observer well acquainted with the mysteries of Cuban government. Nonetheless, for someone who before his political exile had been chief of the Latin American division of the OSS and then head of Latin American area studies at Boston University, Halperin rarely discusses Cuban society or history. Instead, he reduces the revolution to the statements of Fidel and interprets those statements almost wholly in terms of their possible pro- or anti-Soviet implications. Moreover, for someone who suffered at the hands of the witch hunters (he was fired from his tenured position at Boston University), Halperin's judgments of U.S. policy are very mild. United States actions are usually attributed to miscalculation. Only offhand mention is made of the intense and unrelieved economic and political warfare against Cuba. These actions are not treated as important influences on Cuban domestic and foreign policies.

Since he has lived with the revolution rather than having toured it or studied it, Halperin is able to catch its everyday deceptions and self-deceptions; because he left it as an unrequited lover, he can never accept it as it is. Because the revolution could have been so much, its present mortal state (health care and political prisoners, universal education and rationing) torments those who invested so much hope in it, just as its very existence torments its Cold War enemies in Washington.

JULES R. BENJAMIN
University of Rochester

COLIN M. MACLACHLAN and JAIME E. RODRIGUEZ O.
The Forging of the Cosmic Race: A Reinterpretation of Colonial Mexico. Berkeley and Los Angeles: University of California Press. 1980. Pp. xiv, 362. \$25.00.

This work is a noteworthy landmark in the study of Mexican history because it is the first full-sized volume in English to be devoted entirely to a survey of the Mexican colonial era. Previous colonial surveys have been part of general histories of the country. That it is now possible and desirable to dedicate a whole book to such a conspectus is a measure of the advances made in the study of colonial history in Mexico, the United States, and several other countries.

The book has distinct virtues and vices. The main virtue is comprehensiveness. Despite the strong socioeconomic slant of the content, which reflects tendencies in research over the past decade or so, the more accustomed topics—the conquest, colonial administration, and independence, for example—are also included. A welcome addition to past treatments is a chapter on women and the family.

The mass of information presented in the text is not satisfactorily related to the theme of the book, however, as it is proclaimed in the title and elaborated on in the introduction. "Cosmic race" is, of course, a concept of the Mexican historian and educationist José Vasconcelos, one that expresses the emergence in Mexico during and after the colonial centuries of a population blended from European and native American (along with black and Asian) stocks. Like Vasconcelos, Colin M. MacLachlan and Jaime E. Rodríguez O. regard this as a positive process, one that is distinctly Mexican. "No other part of the Spanish Empire attained a comparable integration of peoples and cultures" (p. 3); nor did any other world region. (There is no comparative examination of other regions, but the statement is probably accurate, for Ibero-America at least.)

The trouble is that this promising theme remains largely submerged throughout the text, to resurface fully only in a brief conclusion. For a strong argument of the case, which is certainly possible, constant connections between the historical material and the theme must be presented to the reader; they are not. The book is indeed oddly lacking in integrative themes, even those of narrower scope. Where, for instance, is there a connected discussion of that hoary creature, the seventeenth-century depression? The alert reader may see it ambling by here and there, but nowhere do the authors seize it by the scruff of the neck.

The book, then, is a firm stride toward a single-volume history of colonial Mexico; but there is further to go. In its present form it is a work that will best serve advanced readers seeking well-condensed information on discrete topics. Perhaps in some future edition the authors will achieve a closer integration of theme and facts and so make the book a valuable introduction to the Mexican past for a broader range of readers.

PETER BAKEWELL
University of New Mexico

T. G. POWELL. *Mexico and the Spanish Civil War.* Albuquerque: University of New Mexico Press. 1981. Pp. xiii, 210. \$17.50.

In July 1936, Mexico had the only Latin American government to favor the Spanish Republicans. This book, T. G. Powell's second on Mexico, might have more accurately been entitled, "Mexico's Reaction to the Spanish Civil War." Powell researched a dozen Mexican newspapers, mostly conservative, and employed the archive of the Mexican secretary of foreign relations, which was the only archive he explored. He also used an excellent list of Mexican memoirs, which most historians of the Spanish Civil War have neglected.

Powell describes the complex motives of the Mexicans, who were influenced by the Mexican Revolution of 1910–17. Leftist President Cárdenas was encouraged to help the Spanish Republicans by some Mexican foreign ministry officials as well as by many army officers and labor leaders. The conservative Mexican press was pro-Franco and, at times, sabotaged Cárdenas's policy. A wealthy Spanish business colony in Mexico also backed the Spanish rebel General Franco. Cárdenas feared a possible consolidation among Mexican reactionaries that could threaten his own regime. This was one reason why Cárdenas backed the Republicans and welcomed 40,000 of them to Mexico as refugees after their defeat in 1939. This was also why his party never recognized Franco's regime. The book explains how the Mexican government sent at least 60 airplanes, 20,000 rifles, 50,000 bombs, 200,000 grenades, and \$2.2 million of other war supplies to the republic. It is unclear, however, whether this list is illustrative or almost comprehensive or whether the Mexican archives are incomplete. Three hundred Mexican volunteers fought for the Spanish Loyalists, although how Powell settled on this figure is obscure. From Powell's account, Ramón P. de Negri appears to have been one of the more inept and emotional individuals who ever served as a diplomat, and it is a mystery why Cárdenas should have sent this colorful figure as ambassador to Spain. Following the tradition of Latin American political asylum, Mexican diplomatic compounds in Madrid, Valencia, and Barcelona protected 800 rightist refugees.

Despite his excellent scholarship on Mexican attitudes toward Spain, the author needed to dig still deeper. First, the economic side of the story is ignored. Second, the vital issues of Mexican-Soviet relations during the decisive years from 1936 to 1939 are left vague, except for several clues (pp. 100, 109, 130, 155). Mexico's relations with the Comintern, however, are generally well handled. Unfortunately, Powell's understanding of Spanish conditions is rather weak. He should have at least consulted the third edition (1977) of Hugh Thomas's classic book, the works of Paul Preston and Burnett Bolloten, and José Sánchez's *Reform and Reaction* (1964). Books published in Spain and using European archives were omitted because they were outside the author's chosen topic.

Powell's image of Spain owes more to Stanley Payne than to Gabriel Jackson. Powell basically fears social revolution and anticlericalism. Not everyone would accept his theory that the Spanish Republic could be characterized as totalitarian (p. xii) or that Franco's movement, especially up to 1942, was merely "authoritarian with the trappings of fascism" (p. 115). Powell has been misled, because after 1943 Franco's government did evolve into a kind of

authoritarian dictatorship. As a historian, Powell should have repressed that memory and examined the task at hand, namely Mexico's reaction to the Spanish Civil War. Powell's theories about the 1970s lead this generally fluent writer to compose one of his few jargon-loaded sentences, in which he claims that Spanish fascist leader José Antonio Primo de Rivera "sought a post modern, totalitarian solution of Spain's modernity crisis" (p. 6). Neither José Antonio nor his successor, *Caudillo* Francisco Franco, could have built a mass fascist movement from 1933 to 1942 using this antihistorical, sociological terminology.

Despite this criticism of Powell's image of Spain, the in-depth research on the Mexican story is well worth the price of this excellent monograph. For those interested in the comparative social history of Hispanic culture, Powell's major insight is that in the 1930s the army and the Catholic hierarchy were basically allied in Spain, while at this time in Mexico the army was generally hostile to the church.

ROBERT H. WHEALEY
Ohio University

ROBERT M. CARMACK. *The Quiché Mayas of Utlatlán: The Evolution of a Highland Guatemala Kingdom*. (Civilization of the American Indians Series, number 155.) Norman: University of Oklahoma Press. 1981. Pp. xviii, 435. \$24.95.

The Quiché Mayas of Utlatlán is an encyclopedic compendium of documentary, ethnographic, and archaeological data bearing on the political and, to a somewhat lesser extent, social history of this highland Guatemalan Maya-speaking population. Its content is thorough and up to date, and for anyone interested in the political history of Guatemala's indigenous peoples, the volume unquestionably will be a fundamental reference for at least the next decade. The study unfortunately is flawed by some poorly founded and unconvincing speculations concerning the "evolution" of the preconquest Quiché polity (chap. 12) and the particulars of lineage-group residential distributions at several archaeological sites during pre-Hispanic times (chap. 8). These, together with the somewhat grating misuse of the term "ecology" throughout chapter 4, render it somewhat less than satisfactory as an anthropologically oriented work at times but still fail to detract from its importance and potential utility as a basic reference on Quiché political history.

Robert M. Carmack begins with a useful historical review of research pertaining to the Quiché from the sixteenth century to the present (chap. 2). He next (chap. 3) discusses the question of Quiché origins: whence and when they came to the Guatemalan highlands—issues concerning which a wealth

of linguistic and traditional (documentary) data are convincingly employed to resolve. Complementary archaeological data are disappointingly spare, however, and those available are less than effectively exploited by Carmack or his cited colleagues. Indeed, an uneven mix of masterfully utilized documentary-linguistic source materials bearing on Quiché political history and weakly, often questionably developed archaeological inferences characterizes the work as a whole. Chapters covering the pre- and postconquest history, preconquest social structure, present-day ethnology, and environmental setting of the Quiché are informative and cogently presented. Those dealing with "Settlement Patterns" (chap. 8) and "The Buildings of Utatlán" (chap. 9), however, are rambling and long on unproductive speculation—as contrasted with inference—with neither really contributing much to the study's stated focus, "the development and change of the Utatlán community" (pp. 10–11). Ironically, it is in its overly ambitious attempt to evaluate the processes underlying the development of the Utatlán polity that *The Quiché Mayas* fails seriously. Carmack's concluding discussion of sociopolitical evolution (chap. 12) severely overreaches the legitimate interpretive limits of the data presented in the approximately 350 preceding pages of substantive text. Although those data do chronicle the historical events in the rise and fall of the Utatlán Quiché polity, they explicitly do "not contain a reconstruction of pre-Hispanic Utatlán culture in the traditional ethnohistoric or ethnographic sense. The reader will find no systematic treatment of Quiché economics, kinship, political organization, art, religion, or language" (p. 10). Without the latter and some understanding of the structural-functional transformations that Quiché culture underwent between the founding of Jakawitz (their first organizational center) and the fall of Utatlán, any attempt to discuss the dynamics of Quiché state development must be considered premature.

In fact, elucidation of Utatlán's origins and the factors responsible for emergence of the Quiché polity could have been approached more directly through a coherent program of problem-oriented archaeological investigations at Jakawitz and Utatlán. The philosophy underlying *The Quiché Mayas*, however, appears to have eschewed the directed collection and use of archaeological data to test otherwise formulated hypotheses and instead to have encouraged the gathering of such data in largely opportunistic fashion for the purpose of lending "color" to the documentary sources that at the same time were adjudged reliable means of "interpreting" them. It would profit Carmack and many other Mesoamerican ethnohistorians to keep in mind that documentary sources relay to us what an ancient people would have us believe concerning

their behavior; archaeological ones materially document how they actually did behave.

The briefly summarized research of D. T. Wallace at Utatlán (pp. 220–25) and of J. M. Weeks at Resguardo and Pakamán (pp. 239–46) represent refreshing exceptions to the foregoing pattern. Even their efforts, however, are poorly served by Carmack's sterile preoccupation with identifying the specific archaeological loci (in terms of structural ruins, clusters, and so forth) of Quiché lineages named in documents of the conquest period. I must also comment negatively on the author's uncritical acceptance and presentation of the traditional, documentary source-based reconstruction of northern Maya (Yucatec) prehistory (pp. 380–88). Several papers presented at a 1977 symposium on this subject effectively demolished the traditional scenario that in fact has been under critical attack by Mayanists for almost a full decade (*The Puuc: New Perspectives*, edited by L. Mills [1979]).

Its inept handling of archaeological data and inappropriate focus on sociopolitical evolution aside, *The Quiché Mayas of Utatlán* represents a monumental and important contribution to the literature of Mayan cultural history. Anyone interested in this field will find it worthwhile and informative reading.

JOSEPH W. BALL
San Diego State University

DANIEL H. LEVINE. *Religion and Politics in Latin America: The Catholic Church in Venezuela and Colombia*. Princeton: Princeton University Press. 1981. Pp. xii, 342. Cloth \$22.50, paper \$6.95.

Given the Roman Catholic Church's important historical and current role in Latin America, this study by Daniel H. Levine comes at a most appropriate time. Although the original field research was undertaken from 1971 to 1973, the author devoted almost ten years to the preparation of the book. The result is an excellent treatise of the relation of the church, and Catholicism in general, to politics in Venezuela and Colombia.

The book is divided into four sections. Part 1 deals with the relation between religion and politics in general, particularly in Latin America. It explores the Venezuelan and Colombian milieus as settings for change in the church and examines the development of institutions and issues in each country. Part 2 discusses the leaders of the two churches, focusing on their goals, orientations, and styles of action. Levine analyzes the bishops' views on authority and their visions of the church and examines their social and political stance as it relates to views of the church. Part 3 investigates other institutional levels: Catholic organizations, change in the church from

the perspective of the dioceses, and styles of action at the local level. Part 4 again looks at the process of change in religion and politics, explores the theoretical and methodological implications of the book's analysis, examines the likely future of each church, and sets the patterns in the broader context of Latin America as a whole.

One of Levine's major premises is that church officials' political orientations and actions vis-à-vis the secular world are determined from within: the backgrounds and personal experiences of the bishops, institutional traditions and routines of each national church, and attitudes toward authority and change within the church. Developments in the social and political context of the secular world have less influence on church attitudes.

Levine indicates that change within the church involves the creation of more open, egalitarian forms of association and interaction to replace the traditional authoritarian and hierarchical structures. As these and other experiences spill over to affect church members' views of society as a whole, Catholic social thought extends beyond saving souls and offering charity; it supports efforts to change social structures and is tied to church action. Thus by definition this broader view of pastoral action cannot be separated from politics, especially the expanded view of politics that calls for social justice and a more equitable distribution of wealth, and seriously questions the legitimacy of national institutions under which the gap between the haves and the have-nots continues to widen. By focusing on Venezuela and Colombia, the author demonstrates that such developments contribute to divisions within each church, as well as to differing views between Venezuelan and Colombian church officials.

Levine has written a very important book of sound scholarship and in-depth analysis. By fusing the religious and political dimensions, it goes beyond the standard treatment of the Latin American church and Catholicism. It is recommended for the specialist, the graduate student, and the advanced undergraduate student.

DONALD L. HERMAN
Michigan State University

PAUL OQUIST. *Violence, Conflict, and Politics in Colombia*. (Studies in Social Discontinuity.) New York: Academic Press. 1980. Pp. xiv, 263. \$25.00.

Between 1946 and the middle of the 1960s, Colombia suffered an eruption of widespread killing, primarily in rural areas, in which at least 200,000 people (or roughly 1.5 percent of a population of 13.5 million) died violently, many of them murdered and atrociously mutilated. Many explanations of the Colombian *violencia* have been offered, vari-

ously emphasizing partisan rivalry, institutional factors, class conflict, and diverse economic or cultural features. Paul Oquist's study presents what he calls a "social structure explanation," which, however, includes no detailed or systematic analysis of Colombian social structure. It is really a two-part interpretation, dwelling first on changes in the character of political and social authority as an initial cause of violence and then discussing five of the diverse types of conflict that took place in various localities.

Colombia's political history has been one of intense conflict for control of the state between the two traditional parties directed by the upper classes. In the nineteenth century, interparty struggles were often violent but of relatively little consequence because the state was too weak to affect upper-class interests. In Oquist's terms, the state remained weak, with society being integrated by a strong structure of social domination. According to Oquist, upper-class social control began to weaken as the 1920s and 1930s brought labor organization and strikes, conflicts over land in certain areas, and finally in the 1940s a notable political mobilization of the lower sectors. These challenges, as well as the need "to accommodate Colombian society to capitalist socioeconomic structures" and "to accelerate integration into the world capitalist system," forced the emergence of a stronger state, with extensive powers to control access to credit, the marketing of crops, and so forth (p. 157). Thus in the twentieth century the Colombian system became one of a strong state, with a weaker structure of social domination. Between 1946 and 1950, however, the state partially collapsed because the efforts of each party to establish hegemonic control over it and its agencies had the effect of undermining its authority. As a result, in many localities latent conflicts formerly held in check by the state broke out—struggles for political and economic control at the local level, for the control of land and coffee crops, and between rival neighboring communities.

Most parts of this interpretation are persuasive. The argument that the collapse of the state unleashed local conflicts would have been more convincing, however, if the author had demonstrated the existence of effective central state control of rural areas before the crisis of the 1940s. In fact, the strong state that he describes seems to be strong only in its control of urban labor, credit institutions, and coffee marketing.

Oquist's study is a substantially comprehensive and systematic interpretation, drawing and elaborating upon insights to be found in the existing literature. Oquist tends to minimize his debt to this literature by including a chapter in which he reviews and criticizes other interpretations in a way that in some cases fails to do full justice to them. The central argument about the decline of state author-

ity because of the partisan struggle for hegemony shares some similarities with the analyses of Mauricio Solaún. They differ in that Solaún emphasizes and treats insightfully continuities in Colombian political culture, whereas Oquist rejects cultural analyses as "racial" in character and confines his attention to changes in the character of the political and socioeconomic systems. Oquist's analysis of the various types of local conflict also depends in part upon previous accounts, but he has helped to illuminate all of these subjects with perspectives drawn from extensive interviews as well as with new and useful statistical analyses.

FRANK SAFFORD
Northwestern University

STEVE STEIN. *Populism in Peru: The Emergence of the Masses and the Politics of Social Control*. Madison: University of Wisconsin Press. 1980. Pp. xvi, 296. \$21.50.

It is axiomatic in Peruvian historiography that the year 1931 marked a watershed in the history of that Andean country. Since World War I, demographic change, internal migration, export-led industrial growth, and urbanization profoundly transformed Peru's established economic and social configurations. As the great wave of depression broke on American shores in 1929, these changes coalesced politically into a vast popular upsurge that ruptured the oligarchical structures of the past and introduced new patterns of mass-based politics. To analyze the shape and character of this new "populist" phenomenon Steve Stein turns, with rather mixed results, to the Peruvian election of 1931.

In that election the two principal candidates, the youthful, middle-class reformer Víctor Raúl Haya de la Torre on the left and the charismatic mestizo army colonel Luis M. Sánchez Cerro on the right, bitterly contested for the votes of the newly constituted and politicized working class. Both elected to mobilize the masses "from above" into vertical, populist coalitions, drawing upon the time-honored charismatic, personalistic, and clientelistic devices of a patrimonial polity. The outcome, as Stein puts it, was "a blend of the hierarchical, elitist modes of the past and the new currents of mass society" (p. 6) that served at once to integrate the masses into politics while effectively controlling their potential to trigger violent, revolutionary upheaval.

Stein's argument is neither new nor entirely convincing. It follows the trail of earlier studies of populism that effectively showed how a generation of populist leaders such as Perón and Vargas (to which we may now add Haya) succeeded in blunting the potential for class conflict by constructing highly

personalistic cross-class alliances. What emerged from the conjuncture of forces in 1930 was the remaking of the old-style Latin American *caudillo*, "modernized" into a new populist mold designed to confront a new set of political circumstances.

Stein correctly goes to some lengths to show, in the figures of Haya and Sánchez Cerro, the metamorphosis of this quintessential Latin American public man, the *caudillo*. But inexplicably he omits from his discussion the recent work of Glen Dealy, whose probing analysis of the cultural factors in Latin society producing the *caudillo* would have served to strengthen his case. More importantly he rather overambitiously concludes that the *caudillo* style rather than ideology or program continued, as in the past, to dominate the popular political culture. True, this may well have some validity in the short run in electoral contests, as in Peru in 1931, but it is less convincing in the longer, postelectoral period of populist rule. The lack of longevity of these regimes together with the periodic resort to repression on the part of their populist leaders suggests that the inherent class antagonisms in such movements cannot in the long term be assuaged by the new *caudillo* style alone. The inherently separate and contradictory interests of the working and middle-elite classes at some point assert themselves, ultimately working to undermine and subvert such coalitions. By focusing exclusively on the 1931 election process, Stein never really confronts this paradox, thereby substantially weakening his case.

Stein is at his best, however, when analyzing APRA's political program and style in the 1931 election. The party's ambivalent radicalism, which sought to combine the best elements of both capitalism and socialism, its tempered anti-imperialism, and its corporatist conception of government and society all receive careful scrutiny in the work. Stein also correctly emphasizes the heavy mystical, quasi-religious tone in the movement, heretofore discerned by Francois Bourricaud and Jeffrey Klaiber, which leads him to characterize it suggestively as representing a kind of "political Catholicism" to its followers (p. 175).

Less satisfactory and more troubling is Stein's penchant to accept Aprista mythology as fact. He rather uncritically repeats what the Aprista propagandists say about Haya's supposedly seminal role in such events as the struggle for the eight-hour day in 1919 and the demonstrations against Leguía's bizarre attempt to consecrate the nation to the Sacred Heart of Jesus in 1923—this despite the author's description of the party's ethos as authoritarian, if not totalitarian (fascist), a seemingly powerful disclaimer for any general acceptance of official party accounts of historical events.

PETER F. KLARÉN
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HARRIS GAYLORD WARREN. *Paraguay and the Triple Alliance: The Postwar Decade, 1869–1878*. Assisted by KATHERINE F. WARREN. (Latin American Monographs, number 44.) Austin: Institute of Latin American Studies, University of Texas; distributed by University of Texas Press, Austin. 1978. Pp. 376. Cloth \$17.95, paper \$9.95.

Very little of substance has been written, in any language, on Paraguay after the War of the Triple Alliance (1864–70), for there is a paucity of documents from the epoch. Official papers have been burned, stolen, subjected to the elements, and eaten by local insects. It has always been assumed that the conquered country was a wasteland. Thanks to Harris Gaylord Warren's unusually fine study, we learn that this was indeed the case.

Drawing upon two decades and more of interest and research in Paraguayan history, Warren collected his data from a bewildering variety of private and public repositories, chiefly in Paraguay and Brazil. His research is impeccable, as the copious notes and impressive bibliography attest. He presents us with a genuine tragicomedy in one act, and, being a truly accomplished writer, he does the story justice. Paraguay was a shattered nation in 1870, ground under by foreign occupation and looted with great thoroughness. The only plus in the nation's ledger was the fact that, although the treasury was empty, there was no debt, foreign or domestic.

The bulk of the book details (often sarcastically) how a rogues' gallery of venal Paraguayans and Britons unfortunately resolved that anomaly. In fact, the book is really a history of the sale of Paraguay—a genuine tragedy. The first postwar congress session authorized a loan of one million pounds, to be guaranteed by the nation's only physical asset (save its land), the sometimes functioning national railway. As Warren accurately notes, the London money market in the nineteenth century was truly a thieves' paradise, and Paraguayan agents, whose greed was surpassed only by their naïveté, were quickly "taken." By 1877 "the country was deeply in debt, floundering in a financial morass," and selling (for a pittance) immense tracts of national lands: five million hectares to Argentine Carlos Casado alone (pp. 136–37).

Corruption was endemic and began at the top: "What President Jovellanos and his ministers did not steal outright, they squandered on graft-yielding contracts" (p. 139). Poor Paraguay became poorer, and the mad scramble for money divided the people and helped create the nation's two major political parties (Colorado and Liberal). Occasionally the author displays a cynicism to match his story, as when he writes that "the Paraguayan male, once so

industrious, became a loafer and a bandit" (p. 153).

Not at all ignored is the rivalry for influence of Brazil and Argentina in Paraguay. Brazil was the winner by a nose, and during the occupation is credited with preventing anarchy and moderating Argentina's extreme territorial demands. Paraguay, however, well into the present century "was far from having recovered from the social and economic disaster" of the worst conflict in South America's history.

Warren is at work on a sequel to this volume, which will treat the "Colorado Era," from 1878 to 1904. If it has the qualities of this book, we stand to gain a great deal.

JOHN HOYT WILLIAMS
Indiana State University

MILTON I. VANGER. *The Model Country: José Batlle y Ordoñez of Uruguay, 1907–1915*. Hanover, N.H.: University Press of New England, for Brandeis University Press, Waltham, Mass. 1980. Pp. xii, 436.

This second volume of Milton I. Vanger's life and times of José Batlle y Ordoñez is a major contribution to Uruguayan and Platine history. It subjects Batlle's re-election and second presidency to detailed examination and sheds new light on party politics, legislation, and ideology.

After his auspicious first term, Batlle went to Europe in 1907, kept apart from Uruguayan affairs, and awaited his return. Conservatives who hoped that exile would cool his reformist zeal were disappointed. After four years in Paris, he still championed progressive causes.

The elections that returned him to office in 1911 were not the result of a middle-class-working-class coalition. Few workers voted for Batlle. Since there was little contemporary mention of a middle class, reasons Vanger, such an unobserved group was not important. Instead Batlle appealed to loyal Colorados, whose social base Vanger deems irrelevant.

Batlle then renewed his quest for a model country. He sponsored legislation for divorce and inheritance rights for illegitimate children. His benevolent neutrality in the general strike of 1911 began "a process of winning labor for the Colorado Party" (p. 140). He promoted state enterprises for banking and insurance and tried to assert control over state lands benefiting private individuals. He hoped to guarantee the eight-hour day, state pensions, and better education. Batlle's most controversial reform was his scheme to install a collegiate executive, a program that dwarfed all other issues. This, he hoped, would forestall future dictatorships and guarantee the continuing development of Uruguay under Colorado domination. Instead it split the party.

Those who see Batllista reforms as designed primarily to benefit the middle sectors of urban society, Vanger contends, miss the point; these critics see Batlle from the perspective of Uruguay's current crisis. His goals were truly national; the state would "provide the minimum decencies of life for all" (p. 198). For rural society Batlle intended major transformations—a shift from extensive to intensive ranching and an expansion of the stunted agricultural sector. He kept the military quiescent, initiated mass politics in Uruguay, and fashioned rather than followed public opinion.

Batlle's ideological roots grew from thinkers like Heinrich Ahrens and Henry George. Don Pepe scarcely knew Marx and other socialist theoreticians. An "equalitarian" who believed morality to be inherent in human nature, his "originality lay in his willingness to pursue positions beyond where their first expounders left them . . ." (p. 292).

Vanger refutes most existing class and sector interpretations of Batlle. But his assertion that discerning relationships between class coalitions and policy is an erroneous research strategy must be treated with caution. Batlle's primary appeal was clearly to the Colorado party; still one wishes to know who the Colorados were. Without systematic information about his adherents and opponents, the debate will continue. Its focus, however, will be sharper now.

The author's research is impressive. Archival data, newspapers, interviews, legislative debates, and government documents support this carefully reasoned, clearly written, and altogether worthy study. All will look forward to the concluding volume.

JAMES SCHOFIELD SAEGER
Lehigh University

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

HENRY FRIEDLANDER and SYBIL MILTON, editors. *The Holocaust: Ideology, Bureaucracy, and Genocide*. (The San Jose Papers.) Millwood, N.Y.: Kraus International. 1980. Pp. 361. \$35.00.

DAVID WINSTON, Pagan and Early Christian Anti-Semitism. GAVIN I. LANGMUIR, Medieval Anti-Semitism. EUGEN WEBER, Modern Anti-Semitism. FRITZ K. RINGER, The Perversion of Ideas in Weimar Universities. PETER H. MERKL, The Corruption of Public Life in Weimar Germany. WERNER T. ANGRESS, The German Jews, 1933–1939. RAUL HILBERG, The Anatomy of the Holocaust. RAUL HILBERG, The Significance of the Holocaust. HENRY FRIEDLANDER, The Manipulation of Language. SYBIL MILTON, Artists in the Third Reich. TELFORD TAYLOR, The Legal Profession. GERT H. BRIEGER, The Medical Profession. ALAN BEYERCHEN, The Physical Sciences. THOMAS P. HUGHES, Technology. CHRISTOPHER R. BROWNING, The Government Experts. JOHN S. CONWAY, The Churches. BEATE RUHM VON OPPEN, The Intellectual Resistance. LUCJAN DOBROSZYCKI, Jewish Elites under German Rule. ALLAN MITCHELL, Polish, Dutch, and French Elites under the German Occupation. HENRY L. FEINGOLD, The Government Response. JOHN FELSTINER, The Popular Response. FRANKLIN H. LITTELL, The Credibility Crisis of the Modern University. PAUL M. VAN BUREN, Changes in Christian Theology. JOHN T. PAWLIKOWSKI, Christian Perspective and Moral Implications. LAWRENCE L. LANGER, The Writer and the Holocaust Experience. HENRY FRIEDLANDER, Toward a Methodology of Teaching about the Holocaust.

G. N. CANTOR and M. J. S. HODGE, editors. *Conceptions of Ether: Studies in the History of Ether Theories, 1740–1900*. Cambridge: Cambridge University Press. 1981. Pp. x, 351. \$55.00.

G. N. CANTOR and M. J. S. HODGE, Introduction: Major Themes in the Development of Ether Theories from the Ancients to 1900. P. M. HEIMANN, Ether and Imponderables. J. R. R. CHRISTIE, Ether and the Science of Chemistry:

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HANS FREISING. *Beiträge zur Vor- und Frühgeschichte Mährens*. Edited by WILFRIED FIEDLER. Foreword by HANSJÜRGEN MÜLLER-BECK. Sigmaringen: Jan Thorbecke Verlag. 1980. Pp. 102.

Neue Vorgeschichtsfunde in der Brünner Umgebung. Neue Ergebnisse vorgeschichtlicher Forschungen in Südmähren. Ein keltischer Töpferofen in Südmähren entdeckt. Ein slawisches Skelettgrab in Tschechien. Vor- und frühgeschichtliche Funde aus der Wischauer Sprachinsel. Hallstattische Urnengräber bei Pulgram. Neu entdeckte vor- und frühgeschichtliche Siedlungen im Gerichtsbezirk Nikolsburg. Eine eigenartige germanische Gefäßart aus Südmähren. Keltische Siedlungsreste nächst Joslowitz. Neue germanische Funde um Lundenburg. Vor- und frühgeschichtliche Siedlungsfunde nächst Grafendorf in Südmähren. Spätburgwallzeitliche Funde aus Mönitz. Zur Vor- und Frühgeschichte des Znaimer Ländchens. Die Vorgeschichte der Marktgemeinde Eisgrub. Kelten im Umland der Pollauer Berge. Neue germanische Funde aus dem Bezirk Wischau. Ein germanisches Brandgrab nächst Muschau. Eine jungsteinzeitliche Miniatur-Lockaxt aus Ton. Altsteinzeitliche Funde bei Mödritz. Altsteinzeitliche Funde bei Bratschitz. Ein langobardischer Grabfund aus Némčitz in der Hanna (Mähren). Miozäne Fossilien aus den Diluvialen Sanden von Maloméřitz bei Brünn. Miozäne Strandsande bei Iglau. Vorgeschichtliche Gräber beim Bau Reichsautobahn Breslau-Wien. Mährische Funde und Forschungen im Jahre 1937.

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BARBARA LAMBERT, editor. *Music in Colonial Massachusetts, 1630–1820*. Volume I, *Music in Public Places*. (Publications of the Colonial Society of Massachusetts, number 53; A Conference Held by the Colonial Society of Massachusetts, 1973.) Boston: Colonial Society of Massachusetts; distributed by the University Press of Virginia, Charlottesville, Va. 1980. Pp. xlv, 404.

JOY VAN CLEEF and KATE VAN WINKLE KELLER, Selected American Country Dances and Their English Sources. RAOUL FRANÇOIS CAMUS, Military Music of Colonial Boston. ARTHUR F. SCHRADER, Songs to Cultivate the Sensations of Freedom. CARLETON SPRAGUE SMITH, Broad-sides and Their Music in Colonial America.

ROB KROES, editor. *The American Identity: Fusion and Fragmentation*. (European Contributions to American Studies, number 3.) Amsterdam: Amerika Instituut, Universiteit van Amsterdam. 1980. Pp. 367. f 25.00.

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TRUDY HUSKAMP PETERSON, editor. *Farmers, Bureaucrats, and Middlemen: Historical Perspectives on American Agriculture*. (National Archives Conferences, number 17.) Washington: Howard University Press. 1980. Pp. xvii, 357. \$19.95.

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ics. RENEE M. JAUSSAUD, Staff Note. JONATHAN LURIE, Regulation of the Commodities Exchanges in the 1920s: The Legacy of Self-Government. HAROLD F. BREIMYER, Commodity Marketing: Characteristics and Issues in the Post New Deal Era. ROBERT E. GALLMAN, Commentary. TOM G. HALL, Government Controls: How to Understand the Experience of World War I. RICHARD S. KIRKENDALL, The New Deal for Agriculture: Recent Writings, 1971–76. JAMES T. BONNEN, Observations on the Changing Nature of National Agricultural Policy Decision Processes, 1946–76. GILBERT C. FITE *et al.*, Panel Discussion.

JOSEPH F. STEELMAN, editor. *Of Tar Heel Towns, Shipbuilders, Reconstructionists, and Alliancemen: Papers in North Carolina History*. (East Carolina University Publications in History, number 5.) Greenville, N.C.: East Carolina University Publications. 1981. Pp. xi, 126. \$4.95.

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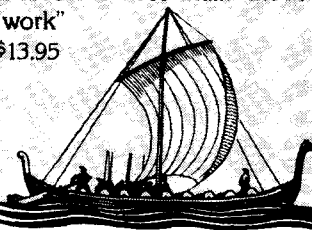
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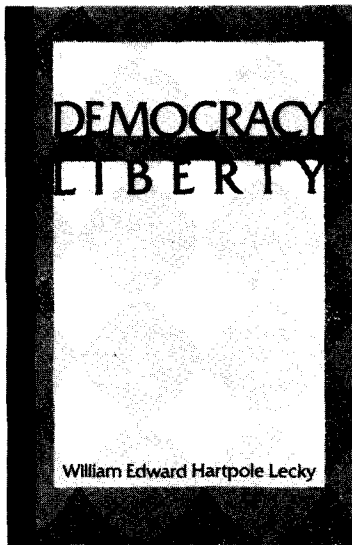
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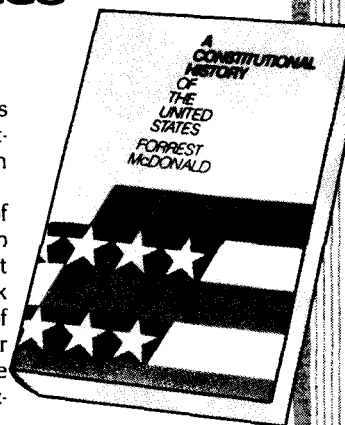
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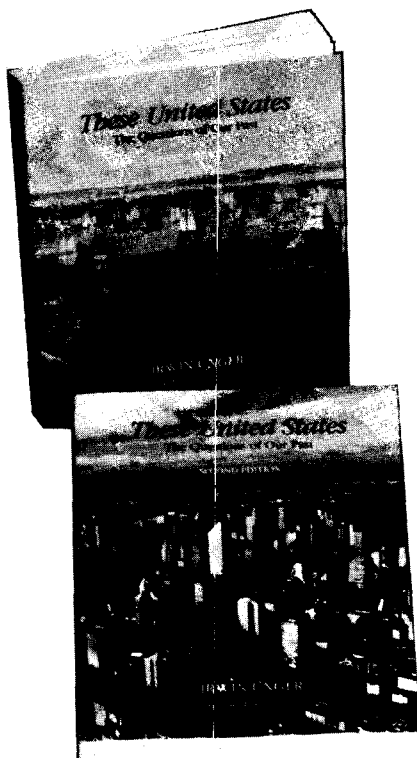
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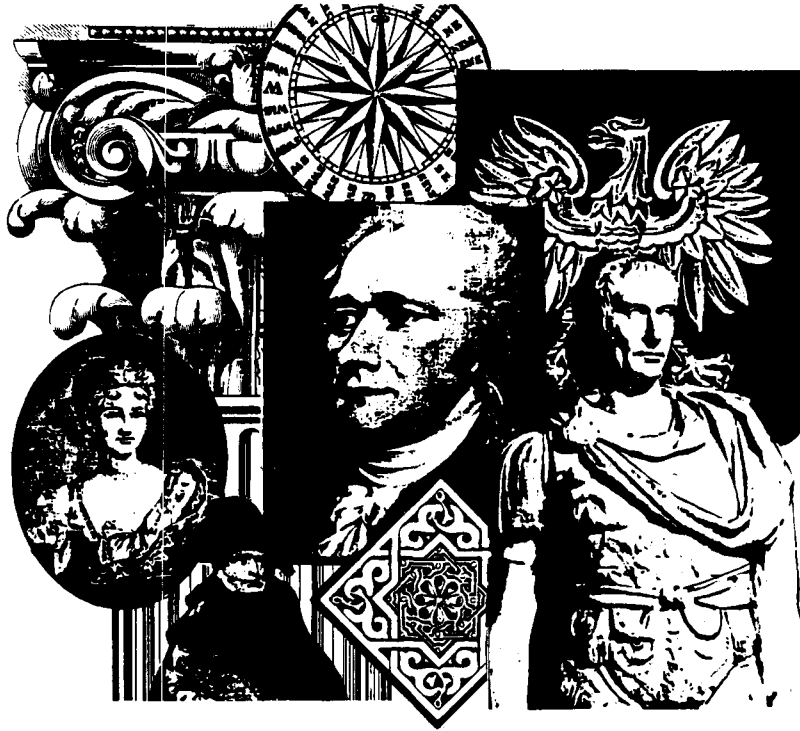
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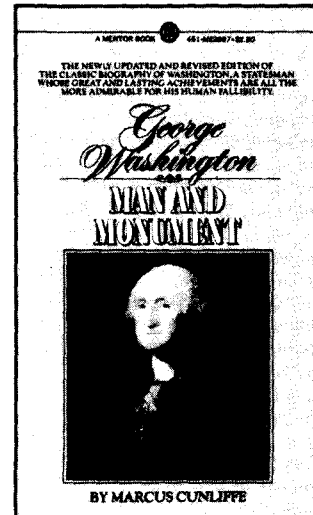
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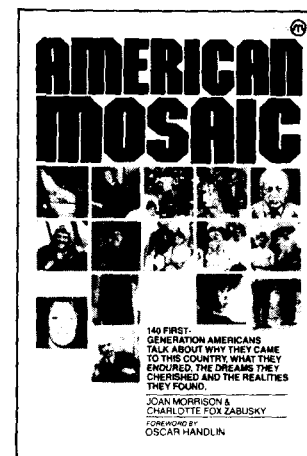


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